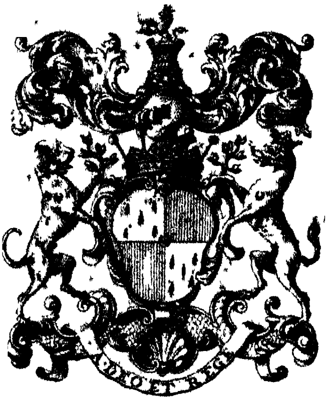
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AGRICULTURAL RESEARCH INSTITUTE
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PHILOSOPHICAL TRANSACTIONS,

OF THE

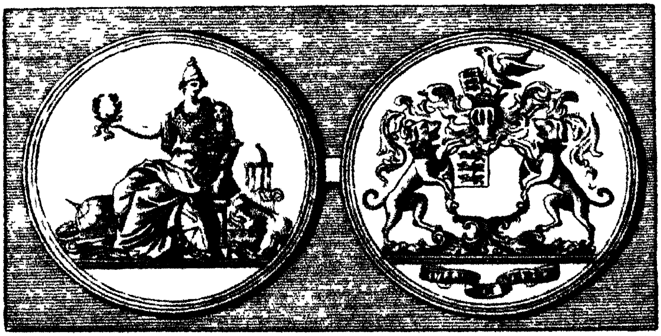
ROYAL SOCIETY

OF

LONDON.

VOL. LXXIV. For the Year 1784.

PART I.



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ADVERTISEMENT.

THE Committee appointed by the *Royal Society* to direct the publication of the *Philoſophical Tranſactions*, take this opportunity to acquaint the Public, that it fully appears, as well from the council-books and journals of the Society, as from repeated declarations, which have been made in ſeveral former *Tranſactions*, that the printing of them was always, from time to time, the ſingle act of the reſpective Secretaries, till the Forty-ſeventh Volume: the Society, as a body, never intereſting themſelves any further in their publication, than by occaſionally recommending the revival of them to ſome of their Secretaries, when, from the particular circumſtances of their affairs, the *Tranſactions* had happened for any length of time to be intermitted. And this ſeems principally to have been done with a view to ſatisfy the Public, that their uſual meetings were then continued for the improvement of knowledge, and benefit of mankind, the great ends of their firſt inſtitution by the Royal Charters, and which they have ever ſince ſteadily purſued.

But the Society being of late years greatly inlarged, and their communications more numerous, it was thought adviſable, that a Committee of their members ſhould be appointed to reconſider the papers read before them, and ſelect out of them ſuch, as they ſhould judge moſt proper for publication in the future *Tranſactions*; which was accordingly done upon the 26th of March 1752. And the grounds of their choice are, and will continue to be, the importance and ſingularity of the ſubjects, or the advantageous manner of treating them; without pretending to anſwer for the certainty of the facts, or propriety of the reaſonings, contained in the ſeveral papers ſo publiſhed, which muſt ſtill reſt on the credit or judgment of their reſpective authors.

It is likewiſe neceſſary on this occaſion to remark, that it is an eſtabliſhed rule of the Society, to which they will always adhere, never to give their opinion, as a body, upon any ſubject, either of Nature or Art, that comes before them. And therefore the thanks, which are frequently propoſed from the chair, to be given to the authors of ſuch papers, as are read at their accuſtomed meetings, or to the perſons through whoſe hands they receive them, are to be conſidered in no other light than as a matter of civility, in return for the reſpect ſhewn to the Society by thoſe communications. The like alſo is to be ſaid with regard to the ſeveral projects, inventions, and curioſities of various kinds, which are often exhibited to the Society; the authors whereof, or thoſe who exhibit them, frequently take the liberty to report, and even to certify in the public news-papers, that they have met with the higheſt applauſe and approbation. And therefore it is hoped, that no regard will hereafter be paid to ſuch reports, and public notices; which in ſome inſtances have been too lightly credited, to the diſhonour of the Society.

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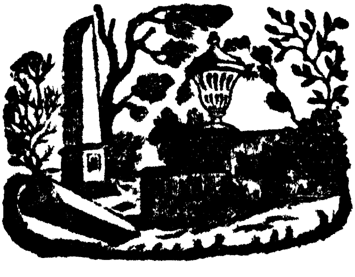


THE Preſident and Council of the Royal Society adjudged, for the laſt Year, 1783, Two Medals on Sir GODFREY COPLEY's Donation; One to JOHN GOODRICKE, Eſq. for his Diſcovery of the Period of the Variation of Light in the Star Algol; and the other to THOMAS HUTCHINS, Eſq. for his Experiments to aſcertain the Point of Mercurial Congelation.

ERRATA.

Page.	Line.	
117.	7.	*for* canon *read* cannon
164.	9.	*for* not *read* nor
181.	8.	*for* called *read* call
211.	7.	*for* Tweed *read* Clyde
224.	4.	*for* of *read* or

PHILOSOPHICAL TRANSACTIONS.

I. *An Obſervation of the Variation of Light in the Star* Algol. *In a Letter from Sir* Henry C. Englefield, *Bart. F. R. S. and S. A. to* Joſeph Planta, *Eſq. Sec. R. S.*

Read November 6, 1783.

SIR, July 3, 1783.

HAVING been fortunate enough, from the fineneſs of the laſt night, to make a ſatisfactory obſervation of the variation of Algol, I loſe no time in communicating it to the Society.

The laſt viſible period was June the 10th, when Mr. AUBERT, as well as myſelf, obſerved it, though imperfectly, and thought the time of its greateſt diminution was about 2¾ h. in the morning; calculating from thence by Mr. Goodricke's period of 2 d. 20 h. 48′, the time of leaſt brightneſs was to be about one o'clock this morning.

All the following obſervations were made with an excellent night-glaſs, magnifying about eight times, with a field of 5°, in which therefore Algol and the ϱ were diſtinctly viſible at once.

I firſt looked out at midnight, and readily found the ſtar, though hardly viſible to the naked eye from the vapours near the horizon. It appeared much bigger than the ϱ, and full as big again as the π, alſo in the field at the ſame time.

At 12½ h. I looked again, and ſaw but little difference, as Algol was then alſo evidently much brighter than ϱ. I at that time faintly perceived it with with the naked eye.

At 1 h. 10′ the ſtar was but very little bigger than ϱ, the diminution having gone on moſt rapidly in the interval between the two laſt obſervations. Though higher above the horizon it was much leſs (if at all) viſible to the naked eye.

At 1 h. 35′ it was, I think, diminiſhed (though but little) ſince the former obſervation. It was ſtill, however, a very little larger than ϱ, but not at all viſible to the naked eye.

At 2 h. it was ſcarce at all altered from the laſt obſervation; but, if any thing, ſeemed recovering its light.

I had meant to obſerve its progreſs ſtill further; but returning to the glaſs at half an hour after two, clouds had ſuddenly covered the whole ſky.

The fact of the diminution of Algol is, however, fully confirmed (if confirmation was wanting) by this obſervation, and the accuracy of the period fixed by Mr. Goodricke aſcertained,

tained, as the phænomenon was certainly within half an hour of the time fixed by Mr. GOODRICKE, which, divided on eight periods, gives only an error of four minutes on the length of it; and a nearer coincidence is not to be expected in a matter of this nature, where eſtimation is the only means of determining the brightneſs, and two perſons can hardly agree within a few minutes, from the difference of ſight.

I am, &c.

II. *Obſervations on the Obſcuration of the Star* Algol, *by* Palitch, *a Farmer. Communicated in a Letter from the Count* de Bruhl, *F. R. S. to Sir* Joſeph Banks, *Bart. P. R. S.*

Read November 13, 1783.

Nov. 7, 1783.
Dover-Street,

SIR,

I AVAIL myſelf of the permiſſion you gave me, when I had the honour to meet you yeſterday in the Drawing-room, by ſending you the following ſhort account, which was tranſmitted to me by Mr. CANZLER, one of the Elector's Librarians, dated Dreſden the 10th of October. PALITCH, a farmer of Prolitz, a village in the neighbourhood of that Reſidence, ſaw the greateſt obſcuration of Algol on the 12th of September, at eight o'clock, P. M. On the 2d and 5th of October he obſerved the ſame phænomenon again. On the 5th the greateſt diminution of that ſtar's light happened ſome minutes before ſeven, when he judged it nearly of the ſize of a ſtar of the fourth magnitude: it continued increaſing in brightneſs till a quarter paſt ten in the evening, at which time it had entirely recovered its uſual brilliancy and ſize. From his own obſervations he eſtimates the period of that remarkable phænomenon at 2 days 20 hours 53 minutes.

I have the honour to be, &c.

III. *Further Obſervations upon* Algol. *By the ſame.*

Read January 15, 1784.

OCT. 20th, PALITCH ſaw Algol nearly at its greateſt obſcuration, at 3 o'clock in the morning.

Oct. 22d, near 12 P. M. he obſerved it again in the ſame ſtate.

Oct. 25th, at about 9 P. M. it appeared to him like a ſtar of the third magnitude. He was prevented by clouds from making long obſervations; but as all thoſe he has had opportunities to make, indicate a period ſomewhat longer than that of 2 days 20 h. 51′ he is inclined to think that half the difference between that period and his own, *viz.* 2 d. 20 h. 52′ will come very near the truth.

IV. *Deſcriptions of the King's Wells at* Sheerneſs, Languard-Fort, *and* Harwich. *By Sir* Thomas Hyde Page, *Knt. F. R. S.*; *communicated by Lieut. Gen.* Rainsford, *F. R. S.*

Read November 13, 1783.

LIEUT. GEN. RAINSFORD.

St. Margaret's-Street,
March 28, 1783.

SIR,

I HAD the pleaſure to receive your obliging letter of the 28th ult. mentioning, that it would be ſatisfactory to the Royal Society to have a deſcription of the wells at Sheerneſs, Harwich, and Languard-Fort, which were made under my direction, whilſt I commanded as Engineer at thoſe places.

I beg to acquaint you, that it will be neceſſary to mention ſome previous circumſtances that occaſioned thoſe undertakings, which will rather interfere with the deſcriptive part, and I fear intrude on the patience of the Society; but I ſhall in this reſpect hope for their indulgence, it being my wiſh to explain the nature of the different operations as fully as poſſible, that ſimilar ſituations, where water is wanted, may receive benefit from the experiments I have had the good fortune to ſucceed in; and it cannot fail of affording me the higheſt ſatisfaction to have an opportunity of communicating this ſubject to the knowledge of the public through the Royal Society.

I have only further to requeſt, that you will do me the honour to lay the following deſcriptions before Sir JOSEPH BANKS and the Society; and as you are fully acquainted with the ſubject, you will confer on me an additional favour by explaining the ſeveral parts (if requiſite) more fully than I have done in the written account, when it is under conſideration.

I am, &c.

Some circumſtances reſpecting the garriſons of Sheerneſs, Landguard-Fort, *and the Town of* Harwich, *with a deſcription of the wells which ſupply water for the uſe of the troops, &c. at each place.*

The Maſter-general of the Ordnance (Lord TOWNSHEND), in the beginning of the year 1778, recommended to his Majeſty, that the fortifications upon the Eaſtern Coaſt, including Dover, Sheerneſs, Landguard-Fort, and ſome other places, ſhould be repaired, and new works added, where they might appear neceſſary towards a proper ſtate of defence, if a war with Holland, or other Northern powers, was found unavoidable. His lordſhip foreſaw the great objection to fortifications, in the want of freſh water under the command of the guns of our garriſons; and I had directions accordingly to conſider the ſubject, and report to his lordſhip and the Board of Ordnance any ideas that might be likely to remedy ſo great a defect.

The dock-yard and garriſon at Sheerneſs were ſupplied with water from Chatham at an enormous expence, near two thouſand

ſand pounds *per annum*, or occaſionally from Queenborough, neither of which ſupplies could be continued in caſe of a ſiege, which of courſe would be of ſhort duration from this circumſtance. Some attempts had been made in former times to obtain water on the ſpot, by ſinking wells, but they had failed; and ſucceſs in ſuch undertakings was at laſt conſidered as impoſſible, from the great difficulty they had met with in the vaſt quantities of ſea-water, that came by filtration through the ſands into their wells, and rendered a progreſs to any conſiderable depth impracticable. It is probable, that the courſe of the river Medway has undergone many changes, and had once an out-fall to the ſea, near the high ground of the Iſle of Shepey. The docks, garriſons, buildings, &c. for a conſiderable diſtance into the iſland, conſequently ſtand upon very looſe materials, which were found, upon ſinking the well in Fort Townſhend, to conſiſt of mud, ſea-beach, and quickſand, nearly to the preſent depth of the river Medway, and admit ſo ſtrong a filtration of ſalt-water, as muſt ever render the ſinking of wells exceedingly difficult. This was the ſituation in which I found Sheerneſs.

Landguard-Fort was not more eligible reſpecting water, as a place of ſtrength. It was, indeed, better ſupplied under any other conſideration, a pipe being laid into the place from a good ſpring about two miles diſtant, which furniſhed a plentiful quantity of water; but ſuch is the diſadvantage of ſituation that, in caſe of attack, that ſpring muſt fall into the poſſeſſion of the enemy, and our garriſon of courſe would be deprived of its uſe. This was a ſerious conſideration and objection to a great extent of fortification, however eligible in other reſpects the place might be.

Harwich was judged by the Commander in Chief (Lord AMHERST) to be a very proper ſtation for a conſiderable part of the army, in time of war with Holland, as centrical to furniſh detachments for ſuch parts of the coaſt as might be in danger, as alſo to cover a very uſeful harbour and increaſing dock-yard; but his lordſhip was ſenſible of the want of wholeſome water in that neighbourhood, and gave particular directions to eſtabliſh ſuch a ſupply for the camp to be formed there, as might appear proper for the health of the troops; and the ſubſequent orders given by General RAINSFORD, who commanded that diſtrict, perfectly anſwered every deſirable end, until good water was found within his camp. The inhabitants of the town of Harwich had chiefly depended on rains for their ſupply, the wells being in general brackiſh from the filtration of ſalt-water. The neighbourhood, to many miles diſtance, was not better furniſhed, there being only ſtagnating water in ponds or ſhallow wells, which were ſupplied from the upper ſurface of the ground; and, whether rendered bad by a mixture of copperas, or other mineral, it was not ſuch as could be given for the uſe of the troops with any degree of prudence or attention to their health, and they were, to avoid dangerous conſequences, furniſhed with water, by General RAINSFORD's order, from the oppoſite ſide of the Manningtree River, by boats employed for that purpoſe, the beginning of the firſt campaign.

I will now endeavour to deſcribe the experiments at each place, beginning with the well in Fort Townſhend at Sheerneſs, which with a reference to the plans will, I hope, render the ſubject ſufficiently intelligible.

King's Well, Fort Townſhend, Sheerneſs.

This undertaking was at firſt conſidered as a mere experiment, the probability of ſucceſs being much againſt it; I however thought the attempt, where a dock-yard of great conſequence to the navy was eſtabliſhed, ſhould be made, and carried as far as it could, with a proper attention to œconomy in laying out the money of the public. Such was my opinion ſignified to the Maſter-general and the Board of Ordnance. I received an anſwer thereto, expreſſive of approbation, and full powers to employ proper perſons, and proceed upon the undertaking.

Theſe previous ſteps ſeemed highly neceſſary, as in all works of difficulty, great confidence is as much required as able workmen or good plans.

The favourable opinion his Majeſty was graciouſly pleaſed to expreſs publicly of the project, when he viſited Sheerneſs, and ſaw the well, tended very much towards its final ſucceſs; and the countenance and ſupport of General Craig, governor of that garriſon, greatly promoted perſeverance in a work of ſuch difficulty.

I employed a very ingenious man, Mr. Cole, engine-maker, of Lambeth, as a chief perſon in this buſineſs, and received every aſſiſtance I expected from his experience and judgement in mechanics; and it is but juſtice to him to expreſs, that the ſucceſs of the work greatly depended on his attention and the able aſſiſtants he procured from diſtant parts of the kingdom.

The greateſt acknowledgement is alſo due to the ability of Lieut. Humfrys, of the Engineers, and Mr. Marshall, the Ordnance-overſeer, who were conſtantly on the ſpot, and carried

ried my orders into execution with the greateſt zeal for the ſucceſs of the undertaking as well as judgement. The mentioning theſe gentlemen's names is, as well as a juſtice to their conduct, to recommend harmony and mutual exertion in any future work of this nature, as, without an equal attention in every one, I ſhould greatly doubt ſucceſs, even admitting the ſame plan to be in all other reſpects ſtrictly attended to, as there would be great difficulty and danger to the lives of the workmen if careleſsly carried on.

The work was begun the 4th of June, 1781, and finiſhed the 4th of July, 1782.

A circle of twenty-two feet diameter was firſt marked out on the ground, and the ſpace excavated to the depth of five feet; after which, pieces of wood, called ribs, upon the curve of a diameter twenty-one feet four inches, and about nine inches ſcantling, were placed, to form a complete circle within the excavated part at the bottom, above which other circles of the ſame nature were placed, and ſupported by upright pieces of ſcantlings, having ſhort boards introduced by the intervals, which afterwards preſſed upon the circles or ribs, between them and the exterior parts. Theſe, when united, formed one frame of wood from the bottom to the top, or rather higher than the excavated ſpace, and prevented the mud of the upper ſurface, which was very ſoft, from falling in upon the workmen. In proceeding deeper, care was taken to prevent the ſinking of the before-mentioned frame by its own weight, in excavating parts only under it till another circle of pieces like the firſt, called ribs, was formed, and uprights, with boards behind, introduced. The diſtance between theſe circles was in the firſt, or upper part of the work, about three feet; but as difficulties increaſed they were placed nearer, and in many parts joined each other without any boards or uprights (as will appear in the ſection of the plan), and continued through the whole of the wooden frame, againſt the weight of the mud, quick-ſand, and ſea-beach, to the depth of thirty-ſix feet.

The reaſon of the circular frames being nearer in ſome parts than in others, aroſe from the greater or leſs quantity of ſalt-water that came through the ſands, &c. and often rendered it impoſſible to ſink under the frame more than the thickneſs of one of the ribs, without danger of blowing up, or of the ſides behind the wood ſlipping with the ſtreams of water, and thereby forcing into the bottom of the well, which in ſinking through very wet quick-ſand is much to be apprehended; and an accident of that nature would entirely deſtroy the work. An attention to the plan will ſhew at what depths the filtration of water was moſt dangerous, and the difficulties at different periods, may be eſtimated by the diſtance of the circles, formed of ribs, from each other, and where they appear to join, it was not without the utmoſt efforts of labour that the work could be carried on. At the depth of thirty-ſix feet the wood-work was finiſhed, and ſix feet deeper a firm foundation of hard blue clay diſcovered. The ſeveral parts of the frame were then ſtrengthened wherever it appeared neceſſary, to prevent ſeparation, and to reſiſt the immenſe preſſure of ſoft mud, quick-ſand, and looſe ſea-beach, which were ſupported by it.

It muſt be obſerved, that the ſalt-water, after proceeding thus far, came in very faſt through all the joints of the frame, and that holes were left on purpoſe in certain parts to let it run into the well, that it might not be confined entirely to the bottom of the work, which, from the weight upon one part only, might have blown, which is ever (as has been obſerved) to be guarded againſt with the utmoſt caution.

The frame being found of ſufficient ſtrength, and the workmen able, by conſtant drawing with four 36-gallon buckets, to keep the bottom of the well dry enough to proceed further, the greateſt difficulty ſeemed to be overcome. The next proceſs was to cut off or ſtop the ſalt-water out entirely; to effect which, a ſmaller circle was deſcribed at the bottom of the well, upon the hard clay already mentioned, of the diameter of eight feet in the clear, round which a curb, or circular frame of wood, was laid, and a brick ſteening, of two bricks thick in tarris, raiſed gradually towards the top of the well, whilſt, as it proceeded upwards, the ſpace between the back of this ſteening and the wooden frame (fixed ſix feet higher) was filled with good tempered clay, four feet thick, and carefully rammed. During this operation, and raiſing the brick-work, with the clay behind it, the water continued to run over them into the center of the well, now reduced to eight feet diameter, and was conſtantly drawn out, to leave the workmen on the ſides ſufficiently dry to raiſe their work until they had reached the top, and conſequently, as it was water-tight, cut off the filtration from the ſea, precautions having been taken to prevent the danger of blowing at the bottom.

The next proceeding appeared more ſimple; but great care was ſtill neceſſary to avoid damaging the foundation of the works already done, as the leaſt crack might have again introduced the ſalt-water. A ſmaller circle than the laſt was therefore deſcribed, and ribs, forming circles of wood, raiſed ſome feet within the brick-work; and others, of the ſame form, were ſunk to the depth of eight feet below the bottom, upon which the ſeveral works already deſcribed reſted. After this a courſe of bricks was carried up within the laſt mentioned ribs or circles, upon a diameter of ſix feet, whereby they became incloſed and joined with the firſt mentioned brick-work, having the clay wall and wooden frame preſſing behind them upon larger diameters. In ſinking lower, ſmall curbs were at certain diſtances (as will appear in the ſection of the plan) placed to ſupport the ſteening, which conſiſted of two ſtretching courſes of bricks, laid ſeparately, and keyed into the clay or back part of the brick-work by rough pieces of ſtone, flint, &c. to prevent a ſlipping or lowering of the ſteening by its own weight. The work was carried on from this period, without any material difficulty or difference in the clay (except the very extraordinary diſcovery of a piece of a tree at the depth of 300 feet from the top of the well, which is ſhewn in the plan) until the appearance of water at 328 feet deep, by a ſmall mixture of ſand in the clay, with oozing of water from it; and at 330 feet deep, upon boring, the whole bottom of the well blew up, and it was with difficulty the workmen eſcaped the torrents of water that followed them, which was mixed with a quick-ſand that roſe forty feet in the bottom of the well, at which height it ſtill remains. The water roſe in ſix hours 189 feet, and in a few days within eight feet of the top of the well. It has ſince been carefully analyzed by a chemiſt, and found perfectly good for every purpoſe; and, it is preſumed, the quantity will be equal to every demand of public and private uſe at that place, as there has been, ever ſince it was firſt diſcovered, a conſtant drawing of water, and it has hitherto been found impoſſible to lower the well more than 200 feet, there has conſequently always been a depth left in water of 130 feet. It is to be remarked, that the water is of a very ſoft quality, and, upon being drawn, has a degree of warmth unuſual in common well-water. It remains yet to be determined whence that warmth proceeds; but as it proved wholeſome, the circumſtance is fortunate for the ſoldiers of the garriſon; as they will not be liable to complaints that are ſo frequent among troops (as often happens at Dover Caſtle) from imprudence in drinking great quantities of very cold well-water.

King's Wells at Landguard-Fort.

They were begun and finiſhed in the year 1782.

The peculiar ſituation of this fort made it very unlikely that ſprings of freſh-water could ever be found, there being great reaſon to think, that the out-fall of the Ipſwich and Manningtree Rivers, which unite before they reach they ſea, was formerly on the Suffolk ſide of the fort, but is now on the Eſſex ſide; and as the garriſon, in ancient writings, is deſcribed to have been built on the Andrew's Sand, there appeared little probability of any filtration of water through it, except that of the ſea. It, however, ſeemed proper to try the poſſibility of ſinking through it, to endeavour to find a hard bottom, ſimilar to that diſcovered at Sheerneſs, freſh-water being of vaſt conſequence to the defence of the place. The work was accordingly begun; but about the ſame time, in making the excavation of a ditch for one of the batteries, at a very few feet from the upper ſurface of the ſand, a ſmall quantity of freſh-water was perceived; and it was chance that led to a diſcovery of its freſhneſs, from one of the labourers happening to taſte it. The circumſtance was reported to me by Mr. ROBERTS, the Adjutant of the Works; and we, upon examining further, found that the quantity of water upon ſinking was conſiderable, and that it appeared perfectly freſh. I then ordered the well-ſinkers to proceed to this depth at another place, where they found a like appearance of good water; and the quantity was ſo great, as to render it very difficult to keep the bottom of the well, at twelve feet deep, dry enough to ſink further. Every exertion was notwithſtanding uſed, and with great labour a well was ſunk to the depth of low water mark at ſpring tides, about eighteen feet from the upper ſurface of the ſand; when, to the ſurprize of every perſon, the water that roſe from the bottom became, on a ſudden, entirely ſalt. This put an end to the work for a time, as it ſeemed impoſſible to penetrate deeper. I then conſidered the matter very differently with my firſt idea, and though the impoſſibility of having a deep well clearly appeared, there remained a proſpect of a ſufficient ſupply of good freſh water. It may now be neceſſary to recollect, that at a very few feet from the ſurface (eight feet) there was good water; that it continued in vaſt quantity almoſt to the ſpring tide low-water-mark, after which the ſalt-water had appeared; I therefore directed ſand to be thrown into the well, to bring it a little above what had been the *loweſt freſh-water* line (twelve feet from the upper ſurface) and then drew the water out which had mixed. After this, the filtration into the well became again perfectly freſh, and in equal quantity to the firſt appearance. This was, therefore, fixed as the greateſt depth (twelve feet) and another well ſunk at forty feet diſtance, with a horizontal brick drain, having holes left in the ſides for filtration, as deſcribed in the plan, to collect the water, and the bottoms of both wells were ſecured with hard materials; that the whole ſupply of water might be reduced to the drain, which is conſtructed to prevent as much as poſſible the mixture of ſand with the water, and is found to anſwer the deſired end. This ſucceſs aroſe from various unexpected circumſtances; but I am yet at a loſs for the cauſe of the freſh water, or whence it comes.

I conceive, that there is a certain diſtance from the ſea, upon every ſandy ſhore, to which the ſalt-water penetrates, where it is forced whilſt the tide is at its greateſt height; and that ſuch water, when ſo far preſſed into the ſands, has an action back towards the ſea again, as *the tide falls*, and continues to have it until another tide makes it revert; this may account for the filtration of ſalt-water a certain way into a country; and that further, from *probably higher ſurfaces*, there may be freſh-water in the ſame continuation of ſands, and the ſeparation diſcoverable to a degree of great accuracy; whether this action of ſalt-water in the ſand, by friction, can render it freſh, or of a leſs degree of ſalt, I will not pretend to judge. I preſume the contrary; but am even under that idea at a loſs to know how ſo much freſh water gets into the ſand at Landguard-Fort, it being ſo entirely ſeparated from the ſpring of the country. It is evident, upon a full conſideration of the ſubject, that the ſea, to the height of *low water*, will penetrate a vaſt diſtance into a ſandy country, by filtration, and to that height *only*, it having ſo far a conſtant preſſure, and no re-action; the water, therefore, being once in the ſand, can never return by the ſame paſſage, the cauſe of its entrance ſtill remaining; whereas in the higher ſurfaces, the *riſe* and *fall* of tides muſt keep it in conſtant movement, and the diſtance of filtration will bear a proportion to the duration of preſſure which gave it original motion. It is probably not ſo eaſy to account for a body of freſh-water being to the depth of twelve feet in the ſand, and in the ſame line, a few feet deeper, the water ſhould be entirely ſalt, and that they do not mix together. Whether the greater ſpecific gravity of the ſalt-water is ſufficient to prevent a mixture with the freſh upon a higher line, I cannot venture to ſay; but the fact of there being a ſeparation is beyond a doubt, and the depths may be aſcertained to a degree of great accuracy. However this may be accounted for, the diſcovery at Landguard-Fort is of very great conſequence to the garriſon; and there is reaſon to think, that in ſimilar ſituations, where water is wanted, an attention to what has been already explained may be found of uſe.

King's Wells at Harwich.

They were begun the 6th of May, 1781, upon General Rainsford's taking the command at that camp, and finiſhed the 29th of September following.

The wells in this neighbourhood, as has already been obſerved, being very ſhallow, and only depending on ſprings from the upper ſurfaces of the ground, have but little water in the ſummer, and the quality of it is very bad. The beſt of the old wells was in the rear of General Rainsford's camp, and was thought of at firſt for the uſe of the troops; but he prudently declined that ſupply. It was imagined, as the water from the upper ſurface was of a bad quality, that the moſt likely way to obtain a better ſpring was to ſink a well from higher ground, and to endeavour to penetrate through a rock which lay a few yards under the level of the country, although the operation might be tedious, upon the chance of cutting a ſpring of better water, that might be unconnected with the land-drains. The experiment anſwered in every reſpect, as there was not a drop of water found till the rock had been entirely cut through, when, upon finding a conſiderable quantity of moiſt ſand, and boring into it, a plentiful ſpring was diſcovered, and has ſupplied the troops ever ſince with very good water. It is probable this ſupply, the ſpring being very powerful, will be found equal to every demand for public and private purpoſes, in the dryeſt ſeaſons. After this ſucceſs, as matter of curioſity, an old well was made deeper, by excavating through the rocks, where a good ſpring was alſo found; but as that well had been originally ſunk from low ground, a great deal of the bad water from the upper drains, &c. mixes with it, and gives it a diſagreeable taſte.

The plans will deſcribe the manner of making theſe wells ſufficiently. I have chiefly dwelt on the deſcriptive part, to recommend, where it is apprehended any mineral or drain from the upper ſurface of lands, by mixing in wells, may hurt the water, the ſinking from the heights, as there are few countries where very good water may not be found, by a proper attention to locality in making wells.

EXPLANATION OF THE PLATES.

Tab. I. fig. 1. Section of the King's Well in Fort Townſhend at Sheerneſs.
2. Plan of the frame and well.
3. Section of the frame AA.
4. Plan of the well.

Tab. II. X. Line of high-water mark.
Y. Line of low-water mark.
Z. Line of low-water at ſpring-tides.

V. *Extract of a Letter from* Edward Pigott, *Eſq. to M.* de Magellan, *F. R. S.*; *containing the Diſcovery of a Comet.*

Read November 27, 1783.

SIR, York, Nov. 22, 1783.

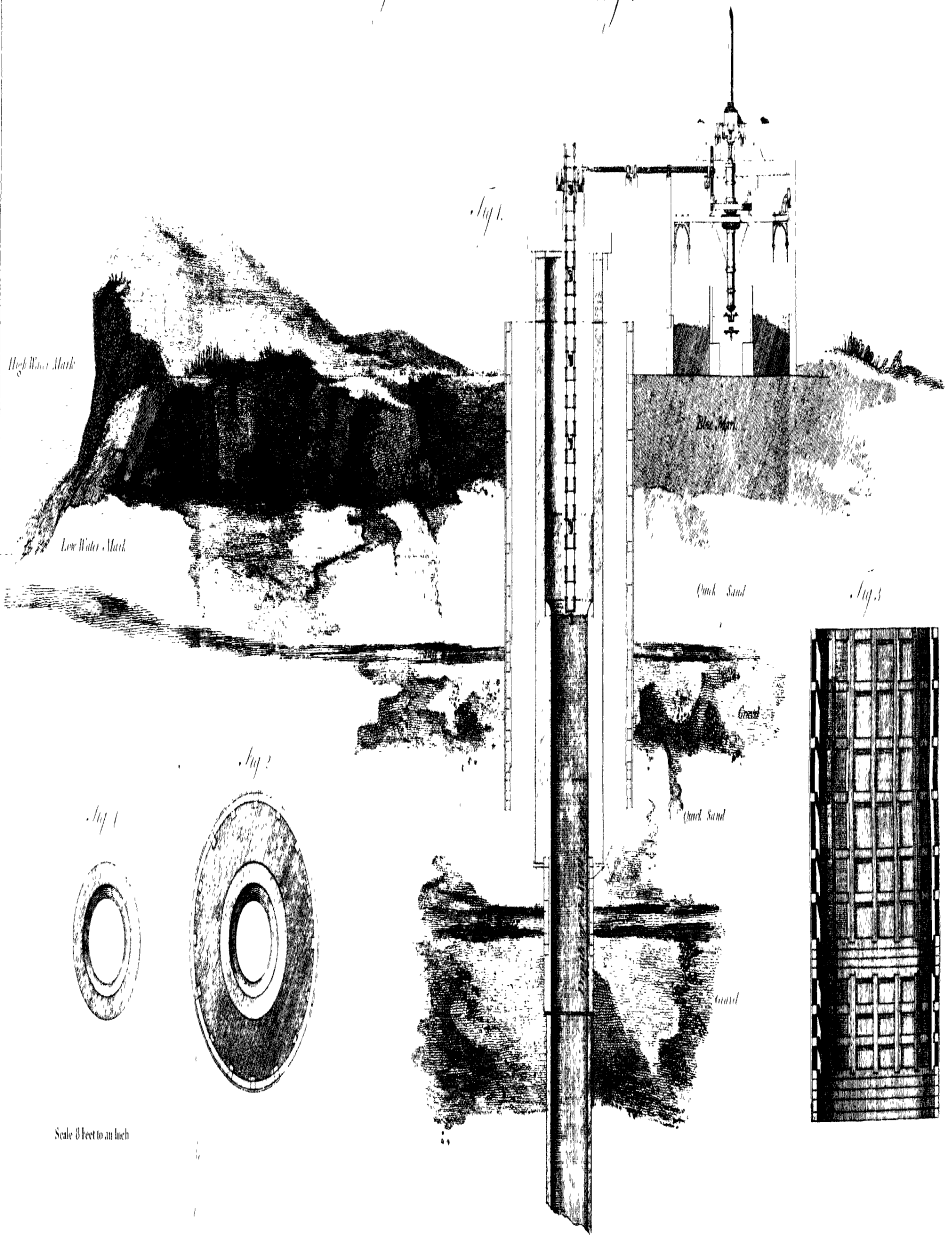
I HAVE the pleaſure of informing you, that I diſcovered a comet on the 19th inſtant, and have made the following obſervations on it.

Date.				R. A.			North Decl.	
1783	h.	′		°	′		°	′
Nov 19	11	15	-	41	0	-	3	10
20	10	54	-	40	0	-	4	32

Nov. 21. This night I ſaw the comet where I expected it, according to the above determinations; but could not obſerve it with an inſtrument.

The comet looks like a nebula, with a diameter of about *two minutes* of a degree. The nucleus being very faint, is ſeen with ſome difficulty, when the wires of the inſtrument are illuminated. It is not viſible with an opera glaſs.

King's Well, Fort Townshend Sheerness.



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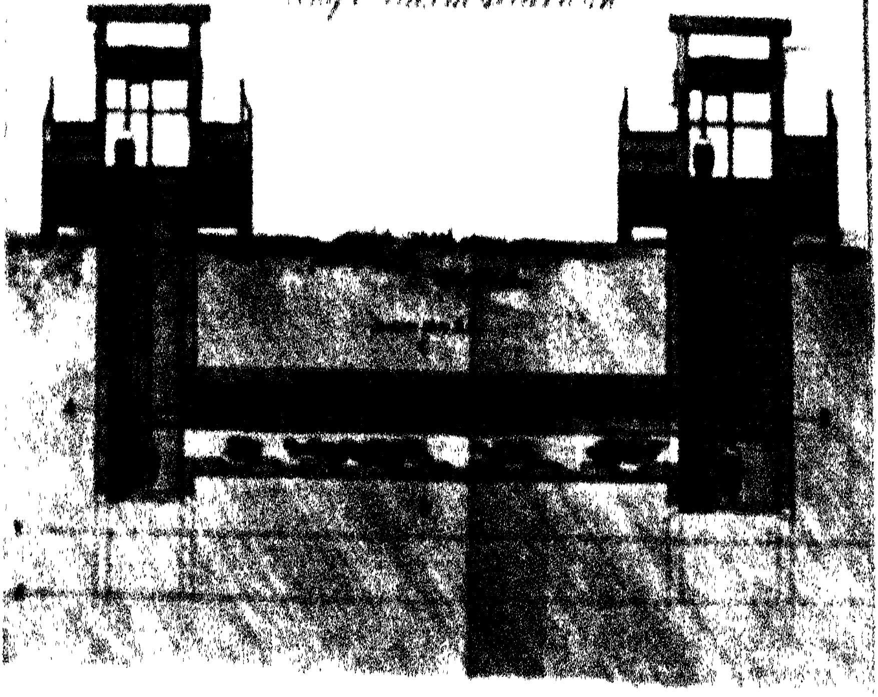

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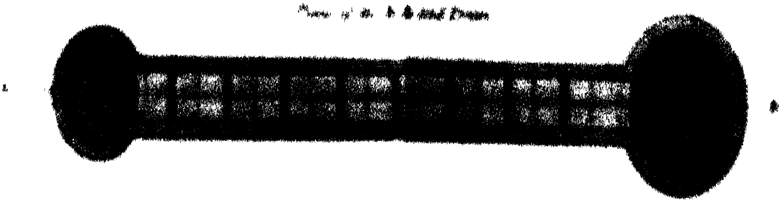

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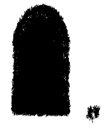

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VI. *Project for a new Diviſion of the Quadrant. By* Charles Hutton, *LL.D. F. R. S. In a Letter to the Rev. Dr.* Maſkelyne, *F. R. S. and Aſtronomer Royal.*

Read November 27, 1783.

DEAR SIR,

Royal Mil. Acad. Woolwich,
Aug. 12, 1782.

HAVING long ſince thought it would be a meritorious and uſeful ſervice, to adapt the tables of ſines, tangents, and ſecants, to equal parts of the radius inſtead of to thoſe of the quadrant; and having frequently mentioned this project to you, SIR, as a proper judge and promoter of all uſeful improvements in ſcience; I now beg leave to lay before you ſome obſervations I have thrown together on the ſubject, with a view to ſtimulate others, either to undertake and calculate ſome part of ſo large and painful a work, or to communicate farther hints for the improvement and eaſier performance of it.

I have the honour to be, &c.

A project for conſtructing ſines, tangents, ſecants, &c. to equal parts of the radius.

1. The arbitrary diviſion of the quadrant of the circle into equal parts by 60ths, which has been delivered down to us from the ancients, and gradually extended by ſimilar ſub-diviſions by the moderns, among various uſes, ſerves for trigonometrical and other mathematical operations, by adapting to thoſe diviſions of the arc, certain lines expreſſed in equal parts of the radius, as chords, ſines, tangents, &c. But among all the improvements in this uſeful branch of ſcience, I have long wiſhed to ſee a ſet of tables of ſines, tangents, ſecants, &c. conſtructed to the arcs of the quadrant as divided into the like equal parts of the radius as thoſe lines themſelves. In this natural way, the arcs would not be expreſſed by diviſions of 60ths, in degrees, minutes, &c., but by the common decimal ſcale of numbers; and the real lengths of the arcs, expreſſed in ſuch common numbers, would then ſtand oppoſite their reſpective ſines, tangents, &c. The uſes of ſuch an alteration would be many and great, and are too obvious and important to need pointing out or enforcing. I have therefore had for a long time a great deſire to commence this arduous taſk; but continual interruptions have hitherto prevented me from making any conſiderable progreſs in ſo deſirable an undertaking. But I am not without hopes that ſome future occaſion may prove more propitious to my ardent wiſhes. It is not, however, to be expected, that this work can be accompliſhed by the labours of one perſon only; it will require rather the united endeavours of many. I ſhall therefore explain a few particulars relative to my project of this work, with a view to obtain from others,

who may have leiſure and abilities for it, their kind aſſiſtance, either by communicating hints of improvements, or by undertaking ſome part of the computations, to which they may be excited by their zeal for the accompliſhment of ſo important a work, and by the extreme facility with which the calculations in this way are made.

2. In the firſt place then I would obſerve, that I think it will be ſufficient to print the ſines, tangents, &c. to ſeven places of figures; and that therefore it will be neceſſary to compute them to ten places, in order effectually to ſecure the truth of the ſeventh place to the neareſt unit.

3. I would aſſume the radius equal to 100000, or ſuppoſe it to be divided into 100000 equal parts. Then it is well known, that the ſemi-circumference will be 314159·26536 nearly, and conſequently the quadrant nearly 157079·63268 of the ſame equal parts, which is leſs than 157080 by ·36732, or nearly $\frac{1}{3}$ of an unit, or nearer $\frac{3}{8}$ = ·375, or nearer $\frac{4}{11}$ = ·3636, or ſtill nearer $\frac{7}{19}$ = ·3684, or ſtill nearer $\frac{11}{30}$ = ·36666 &c. And the half quadrant, or $\frac{1}{8}$ of the circle, 78539·81634 which is leſs than 78540 by only ·18366, or nearly $\frac{1}{2}$ only of any of the above-mentioned fractions.

4. The table may conſiſt of five or more columns; the firſt column to contain the regular arithmetical ſeries of arcs differing by unity, from the beginning, in this manner, 1, 2, 3, 4, 5, &c. up to half the quadrant, the next leſs whole number being 78539; then for the higher numbers, or thoſe in the latter half quadrant, beſides adding 1 continually, there muſt be at the firſt added the decimal ·63268, which will make all the numbers in this half become the exact complements of the firſt half, which conſiſts of whole numbers only; and theſe will be the lengths of the arcs. Or, in order to include the

quadrantal arc 78539·81634, the firſt column may be continued up to 78540. The ſecond column to contain the correſponding degrees, minutes and ſeconds to the neareſt ſecond, or to the true ſeconds and decimals of a ſecond, for the convenience of eaſily changing the tables from the one meaſure to the other, or to make them anſwer to both methods; and the 3d, 4th, 5th, &c. columns to contain the correſponding ſines, tangents, ſecants, &c.

5. The tables may be diſpoſed as at preſent, namely, continuing them downwards by the left-hand ſide of the pages, as far as to the middle of the quadrant, and then returning them again backwards and upwards by the right-hand ſide of the pages.

6. In this diſpoſition, the numbers on the ſame line, the one on the left and the other on the right, will be exact complements of each other to a quadrant, and the decimal ·63268, in every number in the latter half quadrant, in each page, namely, either at the bottom of the column, or length-ways on the ſides of it.

7. A ſpecimen of the firſt page of the table will therefore be this:

0									
Arcs	° ′ ″	Sines.	Tang.	Secants.	Coſec.	Cotan.	Coſin.	° ′ ″	Arcs
0	0 0 0	00000·00	00000·00	100000·00	infin	infin.	100000·00	90 0 0	79
1	2	1·00							78
2	4	2·00							77
3	6	3·00							76
4	8	4·00							75
&c.									&c.
·									
·									
·									
·									
80									
·									
·									
·									
·									
&c.									&c.
96									83
97									82
98									81
99									80
Arcs	° ′ ″	Coſin.	Cotan.	Coſec.	Secants.	Tang	Sines.	° ′ ″	Arcs

+ 632679 &c.

8. To fill up the ſecond column. Since the length of the quadrant is 157079·63267948966, and the number of ſeconds in the quadrant is 90 × 60 × 60 = 324000; therefore, as 157079·632 &c. : 324000 :: 1 : 2·0626480624709 64 = the number of ſeconds anſwering to each unit in our diviſion of the quadrant, and which therefore being continually added will fill up the ſecond column.

9. The number of ſeconds to be continually added being 2, and the decimal ·062648062470964, which is nearly equal to $\frac{1}{16}$, for $\frac{1}{16}$ is ·0625; therefore, beſides adding 2 every time,

we

we muſt alſo add 1″ more at every 16, which will make 3″ to be added at every 16th time, and 2″ at every other time beſides; but the firſt time the 3″ muſt be added will be at the arc or number 8, to have them to the neareſt ſecond, the repetition of the fraction at the arc 8 amounting to above ½ a ſecond; and then the 3″ muſt be added at every 16 afterwards, *viz.* at 24, 40, 56, 72, 88, 104, &c.

10. But beſides the conſtant addition of 2″ every time, and of 1″ more every 16th time, there muſt be 1″ more added for every 6753½ time, on account of the exceſs of the fraction ·062648062470964 over the fraction ·0625 or $\frac{1}{16}$: for that exceſs is ·000148062470964 which is $=\frac{1}{6753\frac{1}{2}}$. And the eaſieſt method of making this laſt addition of 1 at every 6753½, will be to make the increaſe of the 1 on account of the $\frac{1}{16}$ at an unit ſooner for every 422$\frac{3}{32}$; becauſe 16 is 422$\frac{3}{32}$ times contained in 6753½; by which means the incremental units for the $\frac{1}{16}$ will become 1 more at that number 6753½, which laſt unit may be conſidered as the increment of the former increment for the $\frac{1}{16}$, and ſo proceed up to the quadrant; which will complete the ſecond column of arcs to the neareſt ſecond in each number. Or this ſecond column may be exactly computed to as many decimals as we pleaſe, by adding continually the 2″ and decimals, *viz.* 2·062648062470964. But at the middle of the quadrant, where the numbers return again upwards by the right-hand, there will for once be to be added only the ſeconds and decimals anſwering to the arc ·63268, *viz.* 1·30499618 ſeconds, that number being neceſſary to make the numbers on the right-hand to be the exact complements of thoſe on the left. Or it will, perhaps, be proper to make them to the neareſt unit in the 6th place of decimals. And to

fill up the ſecond column to this degree of accuracy, add continually 2·062648 ſeconds, but at the 9th line add 1 more, or 2·062649, becauſe 9 times 062470964 amounts to 56223867, or more than half a unit at that place; and after that add 1 more than 2·062648 at every 16th line, *viz.* at 25, 41, 57, 73, &c. becauſe 16 times 062470964 amounts to ·99953542, or nearly 1, it being only ·00046458 leſs than 1. And this number ·00046458, thus added too much, will, in 134 times adding it, amount to more than 06223867, the exceſs of 56223867 above 5, or half a unit, at that place; therefore at the line or number 2153 (or 9 + 16 × 134) which would be to have the 1 more added, let the 1 be there omitted, and add it at the next line or 2154, the true decimals after the firſt ſix, for 2153 being 499985, and for 2154 they are 562456. Continue thus always adding 1 more at every 16th line, except at the following numbers, where the 1 muſt be omitted, and added at the next following number; *viz.*

2153	10765	19377	27989	36601	45213	53825	62437	71033
4314	12926	21538	30150	38746	47358	55970	64582	73194
6459	15071	23683	32295	40907	49519	58131	66743	75339
8620	17232	25844	34456	43052	51664	60276	68888	77500

And thus proceed to the middle of the quadrant; by which means all the numbers will be to the neareſt unit in the ſixth or laſt place. Alſo, to have a check upon theſe numbers at certain intervals, it may be proper to proceed in this manner: Firſt find every 100th number, by adding its decimal ·204806 &c. verifying them at every 10th; then find every 16th number, by adding continually ·002369 &c. which will alſo be checked and verified at every 25th addition by one of the former ſet of 100, for 25 times 16 make 400, uſing a proper precaution to preſerve each number true to the neareſt unit in the 6th or laſt decimal.

As to the decimals of the numbers in the latter half of the quadrant, they will be the complements, to 1, of the correſponding numbers in the firſt half; and therefore they may be all eaſily found by taking each figure from 9, and the laſt from 10. But it will be ſafeſt to find only every 10th decimal in this way, and to fill up the intermediate nine by adding, as before, the conſtant decimal 062648; by which means they will be checked and verified at every 10th number.

11. To fill up the third column, or that of ſines, as well as thoſe of tangents and ſecants, it may firſt be obſerved, that the old tables of thoſe lines to every minute, or even to every ten ſeconds of the quadrant, cannot be of ſo much uſe as it might ſeem at firſt ſight; as the very near coincidence of the numbers in the new and old diviſions appear very ſeldom to happen. I find, indeed, that our arc 1309 anſwers nearly to 45 minutes, that arc exceeding 45′ by only ·00632363 or $\frac{1}{158}$ part of a ſecond nearly, and ſo in proportion for their equi multiples. But although this degree of coincidence may be ſufficient for checking the correſponding values of the arcs in the firſt and ſecond columns, we are not thereby authoriſed to conſider the ſine, tangent, or ſecant of 1309 as accurately equal to that of 45′ in all the ſeven places of figures, but differing from it by nearly the $\frac{1}{158}$ part of the difference correſponding to 1″, which is about $\frac{1}{4}$ of an unit in the ſines and tangents, though next to nothing in the ſecants. This, therefore, although it makes no ſenſible difference in this particular caſe, will cauſe a difference that muſt not be neglected in the equi-multiples of 1309 and 45′, the ſines and tangents of which will differ by half a unit or

more, and therefore will not be expreſſed by the ſame number, but will have ſome ſmall difference in the ſeventh or laſt figure. And the ſame will happen in almoſt all the other arcs; ſo that generally the ſines, &c. which are exact for the arcs in the firſt column, will not be quite ſo for thoſe in the ſecond, when expreſſed in whole ſeconds only, ſince theſe will ſometimes differ by the part correſponding to almoſt half a ſecond. However, in this, or any other caſe, where the difference is exactly known, we may profitably make uſe of the numbers in the old tables for conſtructing or verifying thoſe of the new, by taking in the proportional part of the difference. Let, therefore, all the ſines, &c. of every 1309 be computed from the old tables, and entered in the new, by adding to the ſine, &c. of the correſponding multiple of 45′ the like multiple of the $\frac{1}{158}$ part of the proportional difference for 1″. This will give about 120 ſines, &c. to ſerve as a verification of the computations by the more general methods. But if the ſecond column be exactly conſtructed with all its decimal places by the continual addition of 2·06264807, the old tables may be converted into the new, by allowing for the odd ſeconds and decimals. And for this purpoſe it will, perhaps, be beſt to uſe the large table of Rheticus, which contains the ſines, tangents, and ſeconds, to ten places of figures for every 10″, and alſo the differences. At leaſt, ſuch ſines, &c. may be found in this way as have their ſeconds and decimals well adapted for the purpoſe; and for ſuch as would be found too troubleſome in this way, recourſe may be had to ſome of the following methods.

12. Let us now examine the expreſſions for the ſines, &c. by infinite ſeries.

The radius being 1, and arc *a*, it is well known that the

ſine is $= a - \frac{1}{6}a^3 + \frac{1}{120}a^5 - \frac{1}{5040}a^7 + \frac{1}{362880}a^9 - \frac{1}{39916800}a^{11}$ &c.

coſine $= 1 - \frac{1}{2}a^2 + \frac{1}{24}a^4 - \frac{1}{720}a^6 + \frac{1}{40320}a^8 - \frac{1}{3628800}a^{10}$ &c.

tang. $= a + \frac{1}{3}a^3 + \frac{2}{15}a^5 + \frac{17}{315}a^7 + \frac{62}{2835}a^9 + \frac{1382}{155925}a^{11}$ &c.

cotang. $= a^{-1} - \frac{1}{3}a - \frac{1}{45}a^3 - \frac{2}{945}a^5 - \frac{1}{4725}a^7 - \frac{2}{93555}a^9$ &c.

ſecant $= 1 + \frac{1}{2}a^2 + \frac{5}{24}a^4 + \frac{61}{720}a^6 + \frac{277}{8064}a^8 + \frac{50521}{3628800}a^{10}$ &c.

coſec. $= a^{-1} + \frac{1}{6}a + \frac{7}{360}a^3 + \frac{31}{15120}a^5 + \frac{127}{604800}a^7 + \frac{73}{3421440}a^9$ &c.

Or the ſame ſeries are thus otherwiſe expreſſed:

ſine $= a - \frac{1}{2 \cdot 3}a^3 + \frac{b}{4 \cdot 5}a^3 - \frac{c}{6 \cdot 7}a^7 + \frac{d}{8 \cdot 9}a^9 - \frac{c}{10 \cdot 11}a^{11}$ &c.

coſine $= 1 - \frac{1}{2}a^2 + \frac{b}{3 \cdot 4}a^4 - \frac{c}{5 \cdot 6}a^6 + \frac{d}{7 \cdot 8}a^8 - \frac{e}{9 \cdot 10}a^{10}$ &c.

tangent $= a + \frac{1}{1 \cdot 3}a^3 + \frac{8b}{4 \cdot 5}a^5 + \frac{17c}{6 \cdot 7}a^7 + \frac{29\frac{3}{17}d}{8 \cdot 9}a^9 + \frac{44\frac{18}{31}e}{10 \cdot 11}a^{11}$ &c.

cotang. $= a^{-1} - \frac{1}{3}a - \frac{b}{15}a^3 - \frac{2c}{21}a^5 - \frac{d}{10}a^7 - \frac{10e}{99}a^9$ &c.

ſecant $= 1 + \frac{1}{2}a^2 + \frac{5b}{12}a^4 + \frac{61c}{150}a^6 + \frac{1385d}{3416}a^8 + \frac{50521e}{124651}a^{10}$ &c.

coſec. $= a^{-1} + \frac{1}{6}a + \frac{7}{60}a^3 + \frac{31c}{294}a^5 + \frac{127d}{1240}a^7 + \frac{2555e}{25146}a^9$ &c.

where *b*, *c*, *d*, *e*, &c. denote the preceding co-efficients. And hence, with the help of the table of the firſt ten powers of the firſt 100 numbers, in p. 101. of my tables of powers publiſhed by order of the Board of Longitude, may be eaſily found the ſines, &c. of all arcs up to 100, by only dividing thoſe powers by their reſpective co-efficients, as alſo of all multiples of theſe arcs by 10, 100, &c. by only varying the decimal points in the ſeveral terms, as the figures will be all the ſame:

and thus a number of primary ſines, &c. may be found, to check or verify the ſame when computed by other methods. By this method will be found the ſines, &c. of the arcs

1, 10, 100, 1000, 10000, 100000;
2, 20, 200, 2000, 20000;
3, 30, 300, 3000, 30000;
4, 40, 400, 4000, 40000;
&c. till
99, 990, 9900, 99000, 990000.

13. Again, it is evident, that, of the terms in the ſeries for the ſine, the firſt term a alone will give the ſine true to the neareſt unit in the ninth place in the firſt 144 ſines, or the arc and ſine will be the ſame for nine places as far as the arc 144; but they will agree to the neareſt unit in the ſeventh place as far as the arc 669; after which the ſecond term of the ſeries muſt be included.

14. When the ſecond term is taken in, theſe two terms $a - \frac{1}{6} a^3$ will give the ſines true to the neareſt unit in the ninth place till the arc becomes 3500. Now the numbers in my table of cubes (juſt publiſhed by order of the Board of Longitude) extend to 10000, and therefore all the above cubes are found in it; conſequently taking the ſixth part of thoſe cubes, and ſubtracting it from the correſponding arcs, the remainders will be the ſines of thoſe arcs, as far as till the arc be 3500: after which the third term of the ſeries may be taken in, or other methods may be uſed.

15. But ſince, for any arc a, this is a general theorem, *viz.* as radius : 2 coſ. a :: ſin. na : ſin. $\overline{n-1} \times a$ + ſin. $\overline{n+1} \times a$; taking $a = 1$, radius 10000, the ſine of a will be $1 - \cdot 0000000000\frac{1}{6}$ and the coſine of a will be $100000 - \cdot 000005$, and the

above proportion will become 100000 : 200000 − ·00001, or 1 : 2 − ·0000000001 :: ſin. n : ſin. $\overline{n-1}$ + ſin. $\overline{n+1}$; conſequently ſin. $\overline{n-1}$ + ſin. $\overline{n+1}$ is = 2 ſin. n. − 0000000001 ſin. n, and the ſines are in arithmetical progreſſion except only for the ſmall difference of ·0000000001 ſin. n, hence ſin. $\overline{n+1}$ is = 2 − ·0000000001 × ſin. n − ſin. $\overline{n-1}$; and therefore taking n ſucceſſively equal to 1, 2, 3, 4, &c. the ſeries of ſines will be as follows:

ſin. 1 = 1 − ·0000000000$\frac{1}{6}$;
ſin. 2 = 2 − ·0000000001 × ſin. 1;
ſin. 3 = 2 − ·0000000001 × ſin. 2 − ſin. 1;
ſin. 4 = 2 − ·0000000001 × ſin. 3 − ſin. 2;
ſin. 5 = 2 − ·0000000001 × ſin. 4 − ſin. 3;
&c.

And by this theorem, *viz.* ſin. $\overline{n+1}$ = $\overline{2 - \cdot 0000000001}$ × ſin. n − ſin. $\overline{n-1}$, may be eaſily filled up the intervals between thoſe primary numbers mentioned in former articles.

16. In like manner, as radius : 2 coſ. a :: coſ. na : coſ. $\overline{n-1}\,.\,a$ + coſ. $\overline{n+1}\,.\,a$; and hence this theorem, coſ. $\overline{n+1}$ = $\overline{2 \quad \cdot 0000000001}$ × coſ. n − coſ. $\overline{n-1}$, by which the coſines will be all eaſily filled up. And theſe two theorems for the ſines and coſines are ſo eaſy and accurate, that we need not have recourſe to any other, but only to check and verify theſe at certain intervals, as at every 100th number, by a proportion from Rheticus's canon, as mentioned at art. 11. or by any other way.

17. The ſines and coſines being compleated, the difference between the radius and coſine will be the verſed ſine; the difference between radius and ſine will be the co-verſed ſine; and the ſum of the radius and coſine will be the ſup.verſed ſine.

18. From the ſines and coſines alſo, the tangents, cotangents, ſecants, and coſecants, may be made by theſe known proportions, *viz.* as

1. coſine : radius :: ſine : tangent,
2. ſine : radius :: coſine : cotangent,
3. coſine : radius :: radius : ſecant,
4. ſine : radius :: radius : coſecant,
5. radius : ſine :: ſecant : tangent,
6. radius : coſine :: coſecant : cotangent,
7. tangent : radius :: radius : cotangent.

Wherefore, the reciprocal of the coſine will be the ſecant; the reciprocal of the ſine, the coſecant; the quotient of the ſine by the coſine, the tangent; and the quotient of the coſine by the ſine, the cotangent; or the product of the ſine and ſecant will be the tangent, and the product of the coſine and coſecant, the cotangent; or, laſtly, the reciprocal of the tangent is the cotangent; proper regard being had to the number of decimals, on account of our radius being 100000 inſtead of 1 only.

And theſe are to be uſed when the application happens to be eaſier than the general ſeries, and eaſier than by proportion from RHETICUS's canon.

But there are other particular theorems, which, by a little addreſs, may be rendered more expeditious than any of the former: thus,

19. In any two arcs this is a general proportion,

As the difference of their ſines :
to the ſum of their ſines ::
ſo tangent of half the difference of the arcs :
to tangent of half their ſum.

So that by taking continually the arcs, having the common difference 2, the third term of this proportion will be 1, and the fourth term will be found by dividing the ſum of the ſines by their difference, which diviſor or difference will never conſiſt of more than four or five figures, *viz.* about half the number of figures that are in the diviſors mentioned in the preceding article.

20. Again, As the difference of the coſines :
to the ſum of the coſines ::
ſo tangent of half their difference :
to tangent of half their ſum.

And thus the cotangents will be found by dividing the ſum of the coſines of two arcs, differing by 2, by their ſmall difference.

21. Alſo the ſecant of an arc is equal to the ſum of its tangent and the tangent of half its complement; and the coſecant of an arc is equal to the ſum of its cotangent and the tangent of half the arc; or half the ſum of the tangent and cotangent is equal to the coſecant of the double arc. From whence the ſecants and coſecants will be eaſily made.

22. Thus I have pointed out methods by which the whole tables may be readily conſtructed. Should any other uſeful methods or improvements occur to any perſon, the communication of them to me will be thankfully received. I am now engaged in making ſome of the computations; and it is hoped, that the facility of them, with the deſireableneſs of the tables, will induce ſome ingenious lovers of the mathematics to lend their aid in performing ſome part of the work. Should any ſuch be ſo inclined, before he begins, I muſt requeſt he will be pleaſed to ſignify his intention to me, that I may point out to him ſuch parts of the work as have not before been performed or undertaken, to prevent the chance of loſing his labour by re-computing any parts that may have been already executed by myſelf or others.

Royal Military Academy,
Woolwich, Aug. 12, 1782.

CHARLES HUTTON.

VII. *On the Means of diſcovering the Diſtance, Magnitude, &c. of the Fixed Stars, in conſequence of the Diminution of the Velocity of their Light, in caſe ſuch a Diminution ſhould be found to take place in any of them, and ſuch other Data ſhould be procured from Obſervations, as would be farther neceſſary for that Purpoſe. By the Rev.* John Michell, *B. D. F. R. S. In a Letter to* Henry Cavendiſh, *Eſq. F. R. S. and A. S.*

Read November 27, 1783.

DEAR SIR, Thornhill, May 26, 1783.

THE method, which I mentioned to you when I was laſt in London, by which it might perhaps be poſſible to find the diſtance, magnitude, and weight of ſome of the fixed ſtars, by means of the diminution of the velocity of their light, occurred to me ſoon after I wrote what is mentioned by Dr. PRIESTLEY in his Hiſtory of Optics, concerning the diminution of the velocity of light in conſequence of the attraction of the ſun; but the extreme difficulty, and perhaps impoſſibility, of procuring the other data neceſſary for this purpoſe appeared to me to be ſuch objections againſt the ſcheme, when I firſt thought of it, that I gave it then no farther conſideration. As ſome late obſervations, however, begin to give us a little more chance of procuring ſome at leaſt of theſe data, I thought it would not be amiſs, that aſtronomers ſhould be apprized of the method, I propoſe (which, as far as I know, has not been ſuggeſted by any one elſe) leſt, for want of being aware of the uſe, which may be made of them, they ſhould neglect to make the proper obſervations, when in their power; I ſhall therefore beg the favour of you to preſent the following paper on this ſubject to the Royal Society.

I am, &c.

THE very great number of ſtars that have been diſcovered to be double, triple, &c. particularly by Mr. HERSCHEL *, if we apply the doctrine of chances, as I have heretofore done in my "Enquiry into the probable Parallax, &c. of the Fixed "Stars," publiſhed in the Philoſophical Tranſactions for the year 1767, cannot leave a doubt with any one, who is properly aware of the force of thoſe arguments, that by far the greateſt part, if not all of them, are ſyſtems of ſtars ſo near to each other, as probably to be liable to be affected ſenſibly by their mutual gravitation; and it is therefore not unlikely, that the periods of the revolutions of ſome of theſe about their principals (the ſmaller ones being, upon this hypotheſis, to be conſidered as ſatellites to the others) may ſome time or other be diſcovered.

2. Now the apparent diameter of any central body, round which any other body revolves, together with their apparent diſtance from each other, and the periodical time of the revolv-

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* See his Catalogue of Stars of this kind, publiſhed in the Philoſophical Tranſactions for the year 1782, which is indeed a moſt valuable preſent to the aſtronomical world. By a happy application of very high magnifying powers to his teleſcopes, and by a moſt perſevering induſtry in obſerving, he has made a very wonderful progreſs in this branch of aſtronomy, in which almoſt nothing of any conſequence had been done by any one before him.

ing body being given, the denſity of the central body will be given likewiſe. See Sir ISAAC NEWTON's Prin. b. III. pr. VIII. cor. I.

3. But the denſity of any central body being given, and the velocity any other body would acquire by falling towards it from an infinite height, or, which is the ſame thing, the velocity of a comet revolving in a parabolic orbit, at its ſurface, being given, the quantity of matter, and conſequently the real magnitude of the central body, would be given likewiſe.

4. Let us now ſuppoſe the particles of light to be attracted in the ſame manner as all other bodies with which we are acquainted; that is, by forces bearing the ſame proportion to their *vis inertiæ*, of which there can be no reaſonable doubt, gravitation being, as far as we know, or have any reaſon to believe, an univerſal law of nature. Upon this ſuppoſition then, if any one of the fixed ſtars, whoſe denſity was known by the above-mentioned means, ſhould be large enough ſenſibly to affect the velocity of the light iſſuing from it, we ſhould have the means of knowing its real magnitude, &c.

5. It has been demonſtrated by Sir ISAAC NEWTON, in the 39th propoſition of the firſt book of his Principia, that if a right line be drawn, in the direction of which a body is urged by any forces whatſoever, and there be erected at right angles to that line perpendiculars every where proportional to the forces at the points, at which they are erected reſpectively, the velocity acquired by a body beginning to move from reſt, in conſequence of being ſo urged, will always be proportional to the ſquare root of the area deſcribed by the aforeſaid perpendiculars. And hence,

6. If ſuch a body, inſtead of beginning to move from reſt, had already ſome velocity in the direction of the ſame line,

when it began to be urged by the aforeſaid forces, its velocity would then be always proportional to the ſquare root of the ſum or difference of the aforeſaid area, and another area, whoſe ſquare root would be proportional to the velocity which the body had before it began to be ſo urged; that is, to the ſquare root of the ſum of thoſe areas, if the motion acquired was in the ſame direction as the former motion, and the ſquare root of the difference, if it was in a contrary direction. See cor. 2. to the abovesaid propoſition.

7. In order to find, by the foregoing propoſition, the velocity which a body would acquire by falling towards any other central body, according to the common law of gravity, let C in the figure (tab. III.) repreſent the centre of the central body, towards which the falling body is urged, and let CA be a line drawn from the point C, extending infinitely towards A. If then the line RD be ſuppoſed to repreſent the force, by which the falling body would be urged at any point D, the velocity which it would have acquired by falling from an infinite height to the place D would be the ſame as that which it would acquire by falling from D to C with the force RD, the area of the infinitely extended hyperbolic ſpace ADRB, where RD is always inverſely proportional to the ſquare of DC, being equal to the rectangle RC contained between the lines RD and CD. From hence we may draw the following corollaries.

8. Cor. 1. The central body DEF remaining the ſame, and conſequently the forces at the ſame diſtances remaining the ſame likewiſe, the areas of the rectangles RC, *r*C will always be inverſely as the diſtances of the points D, *d* from C, their ſides RD, *rd* being inverſely in the duplicate ratio of the ſides CD, C*d*: and therefore, becauſe the velocity of a body falling from an infinite height towards the point C, is always in the

ſub-duplicate ratio of theſe rectangles, it will be in the ſub-duplicate ratio of the lines CD, C*d* inverſely. Accordingly the velocities of comets revolving in parabolic orbits are always in the ſub-duplicate ratio of their diſtances from the ſun inverſely; and the velocities of the planets, at their mean diſtances (being always in a given ratio to the velocity of ſuch comets, *viz.* in the ſub-duplicate ratio of 1 to 2) muſt neceſſarily obſerve the ſame law likewiſe.

9. Cor. 2. The magnitude of the central body remaining the ſame, the velocity of a body falling towards it from an infinite height will always be, at the ſame diſtance from the point C, taken any where without the central body, in the ſub-duplicate ratio of its denſity; for in this caſe the diſtance C*d* will remain the ſame, the line *rd* only being increaſed or diminiſhed in the proportion of the denſity, and the rectangle *r*C conſequently increaſed or diminiſhed in the ſame proportion.

10. Cor. 3. The denſity of the central body remaining the ſame, the velocity of a body falling towards it from an infinite height will always be as its ſemi-diameter, when it arrives at the ſame proportional diſtance from the point C; for the weights, at the ſurfaces of different ſphæres of the ſame denſity are as their reſpective ſemi-diameters; and therefore the ſides RD and CD, or any other ſides *rd* and C*d*, which are in a given ratio to thoſe ſemi-diameters, being both increaſed or diminiſhed in the ſame proportion, the rectangles RC or *r*C will be increaſed or diminiſhed in the duplicate ratio of the ſemi-diameter CD, and conſequently the velocity in the ſimple ratio of CD.

11. Cor. 4. If the velocity of a body falling from an infinite height towards different central bodies is the ſame, when it arrives at their ſurfaces, the denſity of thoſe central bodies muſt be

in the duplicate ratio of their ſemi-diameters inverſely; for by the laſt cor. the denſity of the central body remaining the ſame, the rectangle RC will be in the duplicate ratio of CD; in order therefore that the rectangle RC may always remain the ſame, the line RD muſt be inverſely, as CD, and conſequently the denſity inverſely, as the ſquare of CD.

12. Cor. 5. Hence the quantity of matter contained in thoſe bodies muſt be in the ſimple ratio of their ſemi-diameters directly; for the quantity of matter being always in a ratio compounded of the ſimple ratio of the denſity, and the triplicate ratio of their ſemi-diameters, if the denſity is in the inverſe duplicate ratio of the ſemi-diameters, this will become the direct triplicate and inverſe duplicate, that is, when the two are compounded together, the ſimple ratio of the ſemi-diameters.

13. The velocity a body would acquire by falling from an infinite height towards the ſun, when it arrived at his ſurface, being, as has been ſaid before in article 3d, the ſame with that of a comet revolving in a parabolic orbit in the ſame place, would be about 20,72 times greater than that of the earth in its orbit at its mean diſtance from the ſun; for the mean diſtance of the earth from the ſun, being about 214,64 of the ſun's ſemidiameters, the velocity of ſuch a comet would be greater at that diſtance than at the diſtance of the earth from the ſun, in the ſub-duplicate ratio of 214,64 to 1, and the velocity of the comet being likewiſe greater than that of planets, at their mean diſtances, in the ſub-duplicate ratio of 2 to 1; theſe, when taken together, will make the ſub-duplicate ratio of 429,28 to 1, and the ſquare root of 429,28 is 20,72, very nearly.

14. The ſame reſult would have been obtained by taking the line RD proportional to the force of gravity at the ſun's ſurface, and DC equal to his ſemi-diameter, and from thence computing a velocity, which ſhould be proportional to the ſquare root of the area RC when compared with the ſquare root of another area, one of whoſe ſides ſhould be proportional to the force of gravity at the ſurface of the earth; and the other ſhould be, for inſtance, equal to 16 feet, 1 inch, the ſpace a body would fall through in one ſecond of time, in which caſe it would acquire a velocity of 32 feet, 2 inches per ſecond. The velocity thus found compared with the velocity of the earth in its orbit, when computed from the ſame elements, neceſſarily gives the ſame reſult. I have made uſe of this latter method of computation upon a former occaſion, as may be ſeen in Dr. PRIESTLEY's Hiſtory of Optics, p. 787, &c. but I have rather choſen to take the velocity from that of a comet, in the article above, on account of its greater ſimplicity, and its more immediate connexion with the ſubject of this paper.

15. The velocity of light, exceeding that of the earth in its orbit, when at its mean diſtance from the ſun, in the proportion of about 10.310 to 1, if we divide 10.310 by 20,72, the quotient 497, in round numbers, will expreſs the number of times, which the velocity of light exceeds the velocity a body could acquire by falling from an infinite height towards the ſun, when it arrived at his ſurface; and an area whoſe ſquare root ſhould exceed the ſquare root of the area RC, where RD is ſuppoſed to repreſent the force of gravity at the ſurface of the ſun, and CD is equal to his ſemi-diameter, in the ſame proportion, muſt conſequently exceed the area RC in the proportion of 247.009, the ſquare of 497 to 1.

16. Hence, according to article 10, if the ſemi-diameter of a ſphære of the ſame denſity with the ſun were to exceed that of the ſun in the proportion of 500 to 1, a body falling from an infinite height towards it, would have acquired at its ſurface a greater velocity than that of light, and conſequently, ſuppoſing light to be attracted by the ſame force in proportion to its vis inertiæ, with other bodies, all light emitted from ſuch a body would be made to return towards it, by its own proper gravity.

17. But if the ſemi-diameter of a ſphære, of the ſame denſity with the ſun, was of any other ſize leſs than 497 times that of the ſun, though the velocity of the light emitted from ſuch a body, would never be wholly deſtroyed, yet would it always ſuffer ſome diminution, more or leſs, according to the magnitude of the ſaid ſphære; and the quantity of this diminution may be eaſily found in the following manner: Suppoſe S to repreſent the ſemi-diameter of the ſun, and aS to repreſent the ſemi-diameter of the propoſed ſphære; then, as appears from what has been ſhewn before, the ſquare root of the difference between the ſquare of 497 S and the ſquare of aS will be always proportional to the ultimately remaining velocity, after it has ſuffered all the diminution, it can poſſibly ſuffer from this cauſe; and conſequently the difference between the whole velocity of light, and the remaining velocity, as found above, will be the diminution of its velocity. And hence the diminution of the velocity of light emitted from the ſun, on account of it's gravitation towards that body, will be ſomewhat leſs than a 494.000dth part of the velocity which it would have had if no ſuch diminution had taken place; for the ſquare of 497 being 247.009, and the ſquare of 1 being 1, the diminution of the velocity will be the difference between

the ſquare root of 247.009, and the ſquare root of 247.008, which amounts, as above, to ſomewhat leſs than one 494.000th part of the whole quantity.

18. The ſame effects would likewiſe take place, according to article 11, if the ſemi-diameters were different from thoſe mentioned in the two laſt articles, provided the denſity was greater or leſs in the duplicate ratio of thoſe ſemi-diameters inverſely.

19. The better to illuſtrate this matter, it may not be amiſs to take a particular example. Let us ſuppoſe then, that it ſhould appear from obſervations made upon ſome one of thoſe double ſtars above alluded to, that one of the two performed its revolution round the other in 64 years, and that the central one was of the ſame denſity with the ſun, which it muſt be, if its apparent diameter, when ſeen from the other body, was the ſame as the apparent diameter of the ſun would be if ſeen from a planet revolving round him in the ſame period: let us further ſuppoſe, that the velocity of the light of the central body was found to be leſs than that of the ſun, or other ſtars whoſe magnitude was not ſufficient to affect it ſenſibly, in the proportion of 19 to 20. In this caſe then, according to article 17, the ſquare root of 247.009 SS muſt be to the ſquare root of the difference between 247.009 SS and aaSS as 20 to 19. But the ſquares of 20 and 19 being 400 and 361, the quantity 247.009 SS muſt therefore be to the difference between this quantity and aaSS in the ſame proportion, that is as 247.009 to 222.925,62; and aaSS muſt conſequently be equal to 24.083, 38 SS, whoſe ſquare root 155,2 S nearly, or, in round numbers, 155 times the diameter of the ſun, will be the diameter of the central ſtar ſought.

20. As the ſquares of the periodical times of bodies, revolving round a central body, are always proportional to the cubes of their mean diſtances, the diſtance of the two bodies from each other muſt therefore, upon the foregoing ſuppoſitions, be ſixteen times greater in proportion to the diameter of the central body, than the diſtance of the earth from the ſun in proportion to his diameter; and that diameter being already found to be alſo greater than that of the ſun in the proportion of 155,2 to 1, this diſtance will conſequently be greater than that of the earth and ſun from each other in the proportion of 16 times 155,2, that is 2483,2 to 1.

21. Let us farther ſuppoſe, that from the obſervations, the greateſt diſtance of the two ſtars in queſtion appeared to be only one ſecond; we muſt then multiply the number 2483,2 by 206.264,8, the number of ſeconds in the radius of a circle, and the product 512.196.750 will ſhew the number of times which ſuch a ſtar's diſtance from us muſt exceed that of the ſun. The quantity of matter contained in ſuch a ſtar would be $\overline{155,2}^{3}$ or 3.738.308 times as much as that contained in the ſun; its light, ſuppoſing the ſun's light to take up 8′. 7″. in coming to the earth, would, with its common velocity, require 7.900 years to arrive at us, and 395 years more on account of the diminution of that velocity; and ſuppoſing ſuch a ſtar to be equally luminous with the ſun, it would ſtill be very ſufficiently viſible, I apprehend, to the naked eye, notwithſtanding its immenſe diſtance.

22. In the elements which I have employed in the above computations, I have ſuppoſed the diameter of the central ſtar to have been obſerved, in order to aſcertain its denſity, which cannot be known without it; but the diameter of ſuch a ſtar is

much too ſmall to be obſerved by any teleſcopes yet exiſting, or any that it is probably in the power of human abilities to make; for the apparent diameter of the central ſtar, if of the ſame denſity with the ſun, when ſeen from another body, which would revolve round it in 64 years, would be only the 1717th part of the diſtance of thoſe bodies from each other, as will appear from multiplying 107,32, the number of times the ſun's diameter is contained in his diſtance from the earth, by 16, the greater proportional diſtance of the revolving body, correſponding to 64 years inſtead of 1. Now the 1717th part of a ſecond muſt be magnified 309.060 times in order to give it an apparent diameter of three minutes; and three minutes, if the teleſcopes were mathematically perfect, and there was no want of diſtinctneſs in the air, would be but a very ſmall matter to judge of*.

23. But

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* In Mr. HERSCHEL's Obſervations upon the Fixed Stars abovementioned, almoſt all of them are repreſented as appearing with a well-defined round diſc. That this is not the real diſc, but only an optical appearance, occaſioned perhaps by the conſtitution of the eye, when the pencil, by which objects are ſeen, is ſo exceedingly ſmall as thoſe which he employed upon this occaſion, is very manifeſt, from the obſervations themſelves, of which indeed Mr. HERSCHEL ſeems to be himſelf ſufficiently aware: if it were not ſo, the intenſity of the light of theſe ſtars muſt either be exceedingly inferior indeed to that of the ſun, or they muſt be immenſely larger, otherwiſe they muſt have a very ſenſible parallax; for the ſun, if removed to 10.000.000 times his preſent diſtance, would ſtill, I apprehend, be of about the brightneſs of the ſtars of the ſixth magnitude; in which caſe he muſt be magnified 1.000.000 times to make his apparent diſc of any ſenſible magnitude; or, on the other hand, if he was only removed to a thouſandth part of that diſtance, then he muſt be leſs luminous in the proportion of 1.000.000 to 1, to make him appear no brighter than a ſtar of the ſixth magnitude. Now the ſun's diameter being contained nearly 215 times in the diameter of the earth's orbit, the annual parallax therefore of ſuch a body in that caſe, if it was placed in the pole of the

ecliptic,

23. But though there is not the leaſt probability that this element, ſo eſſential to be known, in order to determine with preciſion the exact diſtance and magnitude of a ſtar, can ever be obtained, where it is in the ſame circumſtances, or nearly the ſame, with thoſe above ſuppoſed, yet the other elements, ſuch as perhaps may be obtained, are ſufficient to determine the diſtance, &c. with a good deal of probability, within ſome moderate limits; for in whatever ratio the real diſtance of the two ſtars may be greater or leſs than the diſtance ſuppoſed, the denſity of the central ſtar muſt be greater or leſs in the ſixth power of that ratio inverſely; for the periodic time of the revolving body being given, the quantity of matter contained in the central body muſt be as the cube of their diſtance from each other. See Sir I. NEWTON's Prin. b. 3d. pr. 8th. cor 3d. But the quantity of matter in different bodies, at whoſe ſurfaces the velocity acquired by falling from an infinite height is the ſame, muſt be, according to art. 12, directly as their ſemi-diameters; the ſemi-diameters therefore of ſuch bodies muſt be in the triplicate ratio of the diſtance of the revolving body; and conſequently their denſities, by art. 11, being in the inverſe duplicate ratio of their ſemi-diameters, muſt be in the inverſe ſextuplicate ratio of the diſtance of the revolving body. Hence if the real diſtance ſhould be greater or leſs than that ſuppoſed, in the proportion of two or three to one, the denſity of the central body muſt be leſs or greater, in the firſt caſe, in the proportion of 64, or in the latter of 729 to 1.

ecliptic, would be 215 times its apparent diameter; and as the bright ſtar in Lyrâ appeared to Mr. HERSCHEL about a third part of a ſecond in diameter, if this was its real diſc, and it was no bigger than the ſun, it would conſequently have an annual parallax in the pole of the ecliptic of about 72″.

24. There is alſo another circumſtance, from which perhaps ſome little additional probability might be derived, with regard to the real diſtance of a ſtar, ſuch as that we have ſuppoſed; but upon which however, it muſt be acknowledged, that no great ſtreſs can be laid, unleſs we had ſome better analogy to go upon than we have at preſent. The circumſtance I mean is the greater ſpecific brightneſs which ſuch a ſtar muſt have, in proportion as the real diſtance is leſs than that ſuppoſed, and *vice verſâ*; ſince, in order that the ſtar may appear equally luminous, its ſpecific brightneſs muſt be as the fourth power of its diſtance inverſely; for the diameter of the central ſtar being as the cube of the diſtance between that and the revolving ſtar, and their diſtance from the earth being in the ſimple ratio of their diſtance from each other, the apparent diameter of the central ſtar muſt be as the ſquare of its real diſtance from the earth, and conſequently, the ſurface of a ſphære being as the ſquare of its diameter, the area of the apparent diſc of ſuch a ſtar muſt be as the fourth power of its diſtance from the earth; but in whatever ratio the apparent diſc of the ſtar is greater or leſs, in the ſame ratio inverſely muſt be the intenſity of its light, in order to make it appear equally luminous. Hence, if its real diſtance ſhould be greater or leſs than that ſuppoſed in the proportion of 2 or 3 to 1, the intenſity of its light muſt be leſs or greater, in the firſt caſe, in the proportion of 16, or, in the latter of 81 to 1.

25. According to Monſ. BOUGUER (ſee his Traité d'Optique) the brightneſs of the ſun exceeds that of a wax candle in no leſs a proportion than that of 8000 to 1. If therefore the brightneſs of any of the fixed ſtars ſhould not exceed that of our common candles, which, as being ſomething leſs luminous than

wax, we will ſuppoſe in round numbers to be only one 10.000dth part as bright as the ſun, ſuch a ſtar would not be viſible at more than an 100dth part of the diſtance, at which it would be viſible, if it was as bright as the ſun. Now becauſe the ſun would ſtill appear, I apprehend, as luminous, as the ſtar Sirius, when removed to 400.000 times his preſent diſtance, ſuch a body, if no brighter than our common candles, would only appear equally luminous with that ſtar at 4000 times the diſtance of the ſun, and we might then begin to be able, with the beſt teleſcopes, to diſtinguiſh ſome ſenſible apparent diameter of it; but the apparent diameters of the ſtars of the leſs magnitudes would ſtill be too ſmall to be diſtinguiſhable even with our beſt teleſcopes, unleſs they were yet a good deal leſs luminous, which may poſſibly however be the caſe with ſome of them; for, though we have indeed very ſlight grounds to go upon with regard to the ſpecific brightneſs of the fixed ſtars compared with that of the ſun at preſent, and can therefore only form very uncertain and random conjectures concerning it, yet from the infinite variety which we find in the works of the creation, it is not unreaſonable to ſuſpect, that very poſſibly ſome of the fixed ſtars may have ſo little natural brightneſs in proportion to their magnitude, as to admit of their diameters having ſome ſenſible apparent ſize, when they ſhall come to be more carefully examined, and with larger and better teleſcopes than have been hitherto in common uſe.

26. With regard to the ſun, we know that his whole ſurface is extremely luminous, a very ſmall and temporary interruption ſometimes from a few ſpots only excepted. This univerſal and exceſſive brightneſs of the whole ſurface is probably owing to an atmoſphære, which being luminous throughout,

and in ſome meaſure alſo tranſparent, the light, proceeding from a conſiderable depth of it, all arrives at the eye; in the ſame manner as the light of a great number of candles would do, if they were placed one behind another, and their flames were ſufficiently tranſparent to permit the light of the more diſtant ones to paſs through thoſe that were nearer, without any interruption.

27. How far the ſame conſtitution may take place in the fixed ſtars we don't know; probably however it may do ſo in many; but there are ſome appearances with regard to a few of them, which ſeem to make it probable, that it does not do ſo univerſally. Now, if I am right in ſuppoſing the light of the ſun to proceed from a luminous atmoſphære, which muſt neceſſarily diffuſe itſelf equally over the whole ſurface, and I think there can be very little doubt that this is really the caſe, this conſtitution cannot well take place in thoſe ſtars, which are in ſome degree periodically more and leſs luminous, ſuch as that in Collo Ceti, &c. It is alſo not very improbable, that there is ſome difference from that of the ſun, in the conſtitution of thoſe ſtars, which have ſometimes appeared and ſometimes diſappeared, of which that in the conſtellation of Caſſiopeia is a notable inſtance. And if thoſe conjectures are well founded which have been formed by ſome philoſophers concerning ſtars of theſe kinds, that they are not wholly luminous, or at leaſt not conſtantly ſo, but that all, or by far the greateſt part of their ſurfaces is ſubject to conſiderable changes, ſometimes becoming luminous, and at other times being extinguiſhed; it is amongſt the ſtars of this ſort, that we are moſt likely to meet with inſtances of a ſenſible apparent diameter, their light being much more likely not to be ſo great in proportion as that of the ſun, which, if removed to four hundred thouſand times

his preſent diſtance would ſtill appear, I apprehend, as bright as Sirius, as I have obſerved above; whereas it is hardly to be expected, with any teleſcopes whatſoever, that we ſhould ever be able to diſtinguiſh a well defined diſc of any body of the ſame ſize with the ſun at much more than ten thouſand times his diſtance.

28. Hence the greateſt diſtance at which it would be poſſible to diſtinguiſh any ſenſible apparent diameter of a body as denſe as the ſun cannot well greatly exceed five hundred times ten thouſand, that is, five million times the diſtance of the ſun; for if the diameter of ſuch a body was not leſs than five hundred times that of the ſun, its light, as has been ſhewn above, in art. 16. could never arrive at us.

29. If there ſhould really exiſt in nature any bodies, whoſe denſity is not leſs than that of the ſun, and whoſe diameters are more than 500 times the diameter of the ſun, ſince their light could not arrive at us; or if there ſhould exiſt any other bodies of a ſomewhat ſmaller ſize, which are not naturally luminous; of the exiſtence of bodies under either of theſe circumſtances, we could have no information from ſight; yet, if any other luminous bodies ſhould happen to revolve about them we might ſtill perhaps from the motions of theſe revolving bodies infer the exiſtence of the central ones with ſome degree of probability, as this might afford a clue to ſome of the apparent irregularities of the revolving bodies, which would not be eaſily explicable on any other hypotheſis; but as the conſequences of ſuch a ſuppoſition are very obvious, and the conſideration of them ſomewhat beſide my preſent purpoſe, I ſhall not proſecute them any farther.

30. The diminution of the velocity of light, in caſe it ſhould be found to take place in any of the fixed ſtars, is the principal phænomenon whence it is propoſed to diſcover their diſtance, &c. Now the means by which we may find what this diminution amounts to, ſeems to be ſupplied by the difference which would be occaſioned in conſequence of it, in the refrangibility of the light, whoſe velocity ſhould be ſo diminiſhed. For let us ſuppoſe with Sir ISAAC NEWTON (ſee his Optics, prop. VI. paragr. 4 and 5) that the refraction of light is occaſioned by a certain force impelling it towards the refracting medium, an hypotheſis which perfectly accounts for all the appearances. Upon this hypotheſis the velocity of light in any medium, in whatever direction it falls upon it, will always bear a given ratio to the velocity it had before it fell upon it, and the ſines of incidence and refraction will, in conſequence of this, bear the ſame ratio to each other with theſe velocities inverſely. Thus, according to this hypotheſis, if the ſines of the angles of incidence and refraction, when light paſſes out of air into glaſs, are in the ratio of 31 to 20, the velocity of light in the glaſs muſt be to its velocity in air in the ſame proportion of 31 to 20. But becauſe the areas, repreſenting the forces generating theſe velocities, are as the ſquares of the velocities, ſee art. 5. and 6. theſe areas muſt be to each other as 961 to 400. And if 400 repreſents the area which correſponds to the force producing the original velocity of light, 561, the difference between 961 and 400, muſt repreſent the area correſponding to the additional force, by which the light was accelerated at the ſurface of the glaſs.

31. In art. 19. we ſuppoſed, by way of example, the velocity of the light of ſome particular ſtar to be diminiſhed in the

ratio of 19 to 20, and it was there obſerved, that the area repreſenting the remaining force which would be neceſſary to generate the velocity 19, was therefore properly repreſented by $\frac{361}{400}$th parts of the area, that ſhould repreſent the force that would be neceſſary to generate the whole velocity of light, when undiminiſhed. If then we add 561, the area repreſenting the force by which the light is accelerated at the ſurface of the glaſs, to 361, the area repreſenting the force which would have generated the diminiſhed velocity of the ſtar's light, the ſquare root of 922, their ſum, will repreſent the velocity of the light with the diminiſhed velocity, after it has entered the glaſs. And the ſquare root of 922 being 30,364, the ſines of incidence and refraction of ſuch light out of air into glaſs will conſequently be as 30,364 to 19, or what is equal to it, as 31,96 to 20 inſtead of 31 to 20, the ratio of the ſines of incidence and refraction, when the light enters the glaſs with its velocity undiminiſhed.

32. From hence a priſm, with a ſmall refracting angle, might perhaps be found to be no very inconvenient inſtrument for this purpoſe: for by ſuch a priſm, whoſe refracting angle was of one minute, for inſtance, the light with its velocity undiminiſhed would be turned out of its way 33″, and with the diminiſhed velocity 35″, 88 nearly, the difference between which being almoſt 2″. 53‴, would be the quantity by which the light, whoſe velocity was diminiſhed, would be turned out of its way more than that whoſe velocity was undiminiſhed.

33. Let us now be ſuppoſed to make uſe of ſuch a priſm to look at two ſtars, under the ſame circumſtances as the two ſtars in the example above-mentioned, the central one of which ſhould be large enough to diminiſh the velocity of its light one twentieth part, whilſt the velocity of the light of the other,

which was ſuppoſed to revolve about it as a ſatellite, for want of ſufficient magnitude in the body from whence it was emitted, ſhould ſuffer no ſenſible diminution at all. Placing then the line, in which the two faces of the priſm would interſect each other, at right angles to a line joining the two ſtars; if the thinner part of the priſm lay towards the ſame point of the heavens with the central ſtar, whoſe light would be moſt turned out of its way, the apparent diſtance of the ſtars would be increaſed 2″. 53‴ and conſequently become 3″. 53‴ inſtead of 1″. only, the apparent diſtance ſuppoſed above in art. 21. On the contrary, if the priſm ſhould be turned half way round, and its thinner part lye towards the ſame point of the heavens with the revolving ſtar, their diſtance muſt be diminiſhed by a like quantity, and the central ſtar therefore would appear 1″. 53‴ diſtant from the other on the oppoſite ſide of it, having been removed from its place near three times the whole diſtance between them.

34. As a priſm might be made uſe of for this purpoſe, which ſhould have a much larger refracting angle than that we have propoſed, eſpecially if it was conſtructed in the achromatic way, according to Mr. DOLLOND's principles, not only ſuch a diminution, as one part in twenty, might be made ſtill more diſtinguiſhable; but we might probably be able to diſcover conſiderably leſs diminutions in the velocity of light, as perhaps a hundredth, a two-hundredth, a five-hundredth, or even a thouſandth part of the whole, which, according to what has been ſaid above, would be occaſioned by ſphæres, whoſe diameters ſhould be to that of the ſun, provided they were of the ſame denſity, in the ſeveral proportions nearly of 70, 50, 30, and 22 to 1 reſpectively.

35. If ſuch a diminution of the velocity of light, as that above ſuppoſed, ſhould be found really to take place, in conſe-

qence

quence of its gravitation towards the bodies from whence it is emitted, and there ſhould be ſeveral of the fixed ſtars large enough to make it ſufficiently ſenſible, a ſet of obſervations upon this ſubject might probably give us ſome conſiderable information with regard to many circumſtances of that part of the univerſe, which is viſible to us. The quantity of matter contained in many of the fixed ſtars might from hence be judged of, with a great degree of probability, within ſome moderate limits; for though the exact quantity muſt ſtill depend upon their denſity, yet we muſt ſuppoſe the denſity moſt enormouſly different from that of the ſun, and more ſo, indeed, than one can eaſily conceive to take place in fact, to make the error of the ſuppoſed quantity of matter very wide of the truth, ſince the denſity, as has been ſhewn above in art. 11. and 12. which is neceſſary to produce the ſame diminution in the velocity of light, emitted from different bodies, is as the ſquare of the quantity of matter contained in thoſe bodies inverſely.

36. But though we might poſſibly from hence form ſome reaſonable gueſs at the quantity of matter contained in ſeveral of the fixed ſtars; yet, if they have no luminous ſatellites revolving about them, we ſhall ſtill be at a loſs to form any probable judgment of their diſtance, unleſs we had ſome analogy to go upon for their ſpecific brightneſs, or had ſome other means of diſcovering it; there is, however, a caſe that may poſſibly occur, which may tend to throw ſome light upon this matter.

37. I have ſhewn in my Enquiry into the probable Parallax, &c. of the Fixed Stars, publiſhed in the Philoſophical Tranſactions for the year 1767, the extremely great probability there is, that many of the fixed ſtars are collected together into groups; and that the Pleiades in particular conſtitute one of

theſe groups. Now of the ſtars which we there ſee collected together, it is highly probable, as I have obſerved in that paper, that there is not one in a hundred which does not belong to the group itſelf; and by far the greateſt part, therefore, according to the ſame idea, muſt lye within a ſphære, a great circle of which is of the ſame ſize with a circle, which appears to us to include the whole group. If we ſuppoſe, therefore, this circle to be about 2°. in diameter, and conſequently only about a thirtieth part of the diſtance at which it is ſeen, we may conclude, with the higheſt degree of probability, that by far the greateſt part of theſe ſtars do not differ in their diſtances from the ſun by more than about one part in thirty, and from thence deduce a ſort of ſcale of the proportion of the light which is produced by different ſtars of the ſame group or ſyſtem in the Pleiades at leaſt; and, by a ſomewhat probable analogy, we may do the ſame in other ſyſtems likewiſe. But having yet no means of knowing their real diſtance, or ſpecific brightneſs, when compared either with the ſun or with one another, we ſhall ſtill want ſomething more to form a farther judgment from.

38. If, however, it ſhould be found, that amongſt the Pleiades, or any other like ſyſtem, there are ſome ſtars that are double, triple, &c. of which one is a larger central body, with one or more ſatellites revolving about it, and the central body ſhould likewiſe be found to diminiſh the velocity of its light; and more eſpecially, if there ſhould be ſeveral ſuch inſtances met with in the ſame ſyſtem; we ſhould then begin to have a kind of meaſure both of the diſtance of ſuch a ſyſtem of ſtars from the earth, and of their mutual diſtances from each other. And if ſeveral inſtances of this kind ſhould occur in different groups or ſyſtems of ſtars, we might alſo, perhaps, begin to

form ſome probable conjectures concerning the ſpecific denſity and brightneſs of the ſtars themſelves, eſpecially if there ſhould be found any general analogy between the quantity of the diminution of the light and the diſtance of the ſyſtem deduced from it; as, for inſtance, if thoſe ſtars, which had the greateſt effect in diminiſhing the velocity of light ſhould in general give a greater diſtance to the ſyſtem, when ſuppoſed to be of the ſame denſity with the ſun, we might then naturally conclude from thence, that they are leſs in bulk, and of greater ſpecific denſity, than thoſe ſtars which diminiſh the velocity of light leſs, and *vice verſâ*. In like manner, if the larger ſtars were to give us in general a greater or leſs quantity of light in proportion to their bulk, this would give us a kind of analogy, from whence we might perhaps form ſome judgment of the ſpecific brightneſs of the ſtars in general; but, at all adventures, we ſhould have a pretty tolerable meaſure of the comparative brightneſs of the ſun and thoſe ſtars, upon which ſuch obſervations ſhould be made, if the reſult of them ſhould turn out agreeable to the ideas above explained.

39. Though it is not improbable, that a few years may inform us, that ſome of the great number of double, triple ſtars, &c. which have been obſerved by Mr. Herschel, are ſyſtems of bodies revolving about each other, eſpecially if a few more obſervers, equally ingenious and induſtrious with himſelf could be found to ſecond his labours; yet the very great diſtance at which it is not unlikely many of the ſecondary ſtars may be placed from their principals, and the conſequently very long periods of their revolutions[*], leave very little room to hope

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[*] If the ſun, when removed to 10.000 000 times his preſent diſtance, would ſtill appear as bright as a ſtar of the ſixth magnitude, which I apprehend to be pretty

that any very great progreſs can be made in this ſubject for many years, or perhaps ſome ages to come; the above outlines, therefore, of the uſe that may be made of the obſervations upon the double ſtars, &c. provided the particles of light ſhould be ſubject to the ſame law of gravitation with other bodies, as in all probability they are, and provided alſo that ſome of the ſtars ſhould be large enough ſenſibly to diminiſh their velocity, will, I hope, be an inducement to thoſe, who may have it in their power, to make theſe obſervations for the benefit of future generations at leaſt, how little advantage ſoever we may expect from them ourſelves; and yet very poſſibly ſome obſervations of this ſort, and ſuch as may be made in a few years, may not only be ſufficient to do ſomething, even at preſent, but alſo to ſhew, that much more may be done hereafter, when theſe obſervations ſhall become more numerous, and have been continued for a longer period of years.

pretty near the truth, any ſatellite revolving round ſuch a ſtar, provided the ſtar was not either of leſs ſpecific brightneſs, or of greater denſity than the ſun, muſt, if it appeared at its greateſt elongation, at the diſtance of one ſecond only from its principal, be between three and four hundred years in performing one revolution; and the time of the revolution of the very ſmall ſtar near α Lyræ, if it is a ſatellite to this latter, and its principal is of the ſame ſpecific brightneſs and denſity with the ſun, could hardly be leſs than eight hundred years, though 37″ the diſtance at which it is placed from it, according to Mr. HERSCHEL's obſervations, ſhould happen to be its greateſt diſtance. Theſe periodical times, however, are computed from the above diſtances, upon the ſuppoſition of the ſtar, that revolves as a ſatellite, being very much ſmaller than the central one, ſo as not to diſturb its place ſenſibly; for if the two ſtars ſhould contain equal, or nearly equal, quantities of matter, the periodical times might be ſomewhat leſs, on account of their revolving about their common centre of gravity, in circles of little more than half as great a diameter as that in which the ſatellite muſt revolve upon the other ſuppoſition.

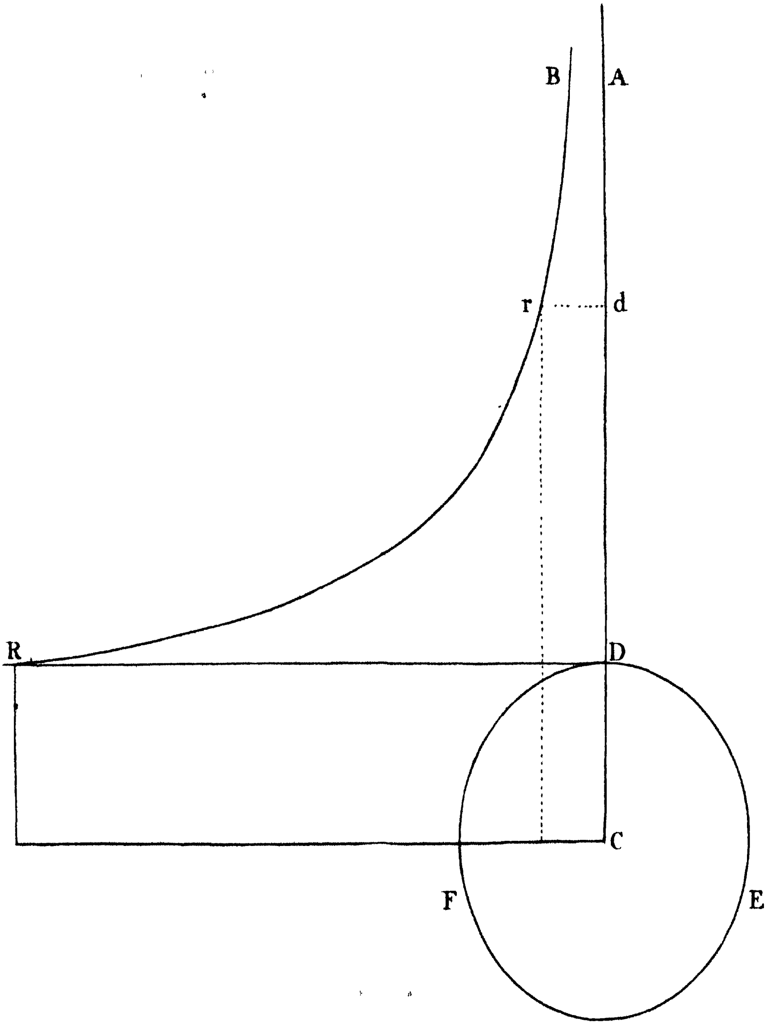
VIII. *A Meteorological Journal for the Year* 1782, *kept at* Minehead, *in* Somerſetſhire. *By Mr.* John Atkins; *communicated by Sir* Joſeph Banks, *Bart. P. R. S.*

Read January 15, 1784.

SIR, Minehead, March 27, 1783.

THE numberleſs philoſophical diſcoveries and meteorological journals which I find have been addreſſed to you, as being a gentleman whoſe great abilities have raiſed you to the higheſt pitch of grandeur in the philoſophical world, and which, I find, have been treated with the greateſt candour and reſpect, and publiſhed under your direction for the improvement of ſcientific knowledge, make me preſume (though an unknown, and even unheard of, individual) to direct this journal to you, not boaſting, but rather doubting, of its being worthy of your reception; but my having found ſo great a difference between the laſt year and ſeveral preceding years, in the variations of the atmoſphere, both barometrical and thermometrical, induced me to communicate it to other obſervers through your approbation.

I am, &c.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THE inſtruments are kept at a houſe about thirty feet above high water in the Briſtol Channel. The barometer is made after de Luc's method; and to obſerve the moſt minute alteration I have divided it into the one-ſixteenth of a line, or 192 parts in an inch. The thermometer is a mercurial one, graduated according to Fahrenheit's ſcale, as being the moſt univerſal, though, I think, a partial one, and placed in the open air in a northern aſpect. An hygrometer I have likewiſe kept in the open air; but being an inſtrument that does not admit of, as I ever heard of, a certain baſis, whereon to fix the fundamental point between the greateſt moiſture and greateſt drought, and therefore of little uſe to diſtant obſervers, I have omitted theſe obſervations. For the eaſe of correſpondent obſervers, I have drawn two columns of the barometer; the firſt divided into 192d parts of an inch; the ſecond into 100th parts. The figures in the column of winds denote its ſtrength from 0 to 90 degrees of a quadrant. And the moſt prevailing winds are from north to weſt, being generally in thoſe directions two-thirds of the year, occaſioned, as I imagine, by the indraught of the Briſtol Channel. The barometer this year has taken a greater range than ever I found theſe ſeveral years, being 2.44 inches. The thermometer likewiſe from 21° to 81°; and the three rainy months of October, November, and December, there fell very little more rain than fell in the month of Auguſt alone, which is very uncommon in this part of the kingdom. On the ninth of February an odd phænomenon appeared to me about 10 miles from hence, on my journey to Tiverton. I obſerved an halo, exactly ſimilar to that of the ſun, the center of the arch about 15° high, and both ends terminated in a field of ſnow; but as rainbows are ſeen only with the ſun behind one's back, this, on the contrary, was between me and the ſun.

January 1782.

Day.	Hour.	Weather.	Winds.	Clouds.	Barom. 192 parts.	Barom. 100 parts.	Thermom.	Rain.
					Inches.	Inches.	°	Inch.
1	8	Hard rain.	SW 25		29. 96	29.50	50	
	12				29.100	29.52	52	
	9		W 20		29.104	29.54	50	0.55
2	8	Rain.	W 30		29. 16	29. 9	48	
	12				29.120	29.63	50	
	9				29.128	29.67	50	0.16
3	8	Fair.	WNW 35		29.120	29.63	48	
	12	Some ſhowers.			29.140	29.73	50	
	9				29.152	29.79	48	
4	8	Foggy, rain.	WNW 10		29.160	29.84	50	
	12				29.140	29.73	54	
	9				29.128	29.67	49	0.17
5	8	Showers.	WNW 27		29.132	29.69	50	
	12				29.150	29.78	51	
	9				29.170	29.89	50	0.25
6	8	Fair.	WNW 22		29.184	29.94	46	
	12	Cloudy.			30. 0	30. 0	50	
	9				30. 0	30. 0	49	
7	8	Hard ſhowers.	NW 40		29.112	29 59	45	
	12				29 120	29.63	50	
	9	Stormy, hail ſhowers.	70		29.128	29.67	42	
8	8	Fair.	WSW 20		29.158	29.83	44	
	12	Showers.			29.150	29.78	50	
	9				29.128	29 67	48	0.50
9	8	Rain.	S 40		29. 0	29. 0	50	
	12		WNW 50		28.168	28.88	40	
	9	Very ſtormy.	NW 80		29. 40	29.21	38	

January 1782.

Day.	Hour.	Weather.	Winds.	Clouds.	Barom. 192 parts.	Barom. 100 parts.	Thermom.	Rain.
					Inches.	Inches.	°	Inch.
	8	Very ſtormy.	NWbyW 80		29.128	29.67	37	
10	12				29.180	29.94	40	0.49
	9	Freezing hard.			30. 48	30.25	35	
	8	Hard froſt and fair.	NW 0		30. 48	30.25	29	
11	12				30. 48	30.25	32	
	9				30. 48	30 25	35	
	8	Foggy rain.	NW 0		30. 40	30.21	45	
12	11				30. 40	30.21	48	
	9				30. 64	30.33	49	
	8	Cloudy.	WNW 0		30. 70	30.37	47	
13	12	Foggy rain.			30. 70	30.37	51	
	9				30. 67	30.35	50	
	8	Fair.	S by W 15		30. 56	30.29	45	
14	12				30. 40	30 21	50	
	9				30. 24	30.13	41	
	8	Froſty, but foggy.	NNW 35		30. 20	30.11	40	
15	12				30. 26	30.15	44	
	9				30. 16	30. 9	44	
	8	Small rain.	WNW 10		29.128	29.67	47	
16	12				29.106	29 55	50	
	9	Stormy, hail ſhowers.	NW 75		29 80	29.42	42	
	8	Stormy, but dry.	NW 70		29. 88	29.46	39	
17	12				29. 96	29.50	43	
	9				29.112	29.59	45	
	8	Fair.	SW 20		29.118	29.61	48	
18	12	Some ſmall rain.			29.126	29.66	48	
	9				29.144	29.75	45	

January

January 1782.

Day.	Hour.	Weather.	Winds.	Clouds.	Barom. 192 parts.	Barom. 100 parts.	Thermom.	Rain.
					Inches.	Inches.	°	Inch.
19	8	Cloudy.	WNW 15		29.140	29.73	45	
	12	Fair and mild.			29.140	29.73	51	
	9	Some ſhowers.			29.130	29.68	46	
20	8	Very hard rain.	W 60		29. 90	29.47	50	
	12				29. 80	29.42	52	
	9				29. 86	29.45	48	0.66
21	8	Froſt.	WNW 10		29.140	29.73	38	
	12	Some rain, but ſnow on the hills.			29.168	29.88	45	
	9				30. 0	30. 0	48	
22	8	Flying clouds, with ſome ſhowers.	W 60		29.180	29.94	51	
	12				29 180	29.94	51	
	9	Cloudy and mild.			29.180	29.94	50	
23	8	Fair and mild.	W 60		29.184	29.96	50	
	12				29.180	29.94	51	
	9				29.176	29.92	50	
24	8	Fair, with hard winds, but very mild.	W 70		29.120	29.63	50	
	12				29.112	29 59	55	
	9	Calm, with rain.	NW 10		29.116	29.61	49	
25	8	Stormy, hail ſhowers.	NW 75		29 120	29.63	36	
	12				29.130	29.68	39	
	9	Rain.	WNW 60		29.100	29.52	44	
26	8	Fair.	NW 60		29 116	29.61	39	
	12	Hail ſhowers.			29.110	29.57	41	
	9	Stormy, hard rain.	70		29.100	29.52	40	
27	8	Stormy, hail and rain.	N by W 60		29. 48	29.25	38	0.93
	12				29. 40	29.21	42	
	9		75		29. 48	29.25	40	

January 1782.

Day.	Hour.	Weather.	Winds.	Clouds.	Barom. 192 parts.	Barom. 100 parts.	Therm.	Rain.
					Inches.	Inches.	°	Inch.
28	8	Stormy, ſhowers.	NWbyW 80		29. 32	29.17	40	
	12				29. 16	29. 9	45	
	9	Calmer, with hail in the night.	20		29. 16	29. 9	40	0.23
29	8	Fair and cold.	W 20		28.176	28.92	33	
	12	Fair.	NW		28.160	28.84	41	
	5		SW		28.154	28.81		
	9	Cloudy.	NE	SW	29. 0	29. 0	37	
30	8	Cloudy, but ſnow on the diſtant hills.	NE 20		29. 64	29.34	37	
	12	Fair.			29.112	29.59	40	
	9	Freezing hard.			29.168	29.88	35	
31	8	Froſt.	NE 15		30. 16	30. 9	33	
	12				30. 16	30. 9	37	
	9				30. 16	30. 9	32	
							Total rain	3.94

February 1782.

Day.	Hour.	Weather.	Winds.	Clouds.	Barom. 192 parts.	Barom 100 parts.	Thermom.	Rain.
					Inches.	Inches.		Inch.
1	7	Very hard froſt.	W 0		30. 0	30. 0	27	
	12	Fair.	SW 20		29.180	29.94	34	
	9	Sleet.			29.160	29 84	37	
2	7	Rain.	SW 30		29.130	29.68	40	
	12				29 120	29.63	42	
	9	Fair.			29.112	29.59	38	
3	7	Froſt.	S by E 20	SW	29. 96	29.50	35	
	12	Sleet.	SW		79. 84	29.44	37	
	9				29. 80	29.42	37	
4	7	Fair.	W 0		29. 80	29.42	37	
	12				29. 88	29.46	38	
	9	Snow on the hills.	NE 20		29. 60	29.31	37	
5	7	Cloudy day.	NE 40		29. 12	29. 6	33	0.63
	12				29. 20	29.11	35	
	9				29. 96	29.50	35	
6	7	Fair, froſt.	NE 20		29.140	29.73	35	
	12				29.160	29.84	37	
	9		W 0		29.168	29.88	37	
7	7	Fair, hard froſt.	W 0		29.170	29.89	29	
	12			ENE	29.172	29.90	35	
	9		NE 10		29.180	29 94	35	
8	7	Fair, froſt.	NE 0		29.180	29.94	34	
	12				29.180	29.94	37	
	9				29.186	29.97	33	
9	7	Fair, very hard froſt; an extraordinary halo.	NE 10		29.182	29.95	27	
	12	Cloudy.			29.180	29.94	33	
	9				29.180	29.94	33	

February 1782.

Day.	Hour.	Weather.	Winds.		Clouds.	Barom. 192 parts.	Barom. 100 parts.	Thermom.	Rain.
						Inches.	Inches.		Inch.
10	7	Cloudy, hard froſt.	NE	15		29.180	29.94	29	
	12	Fair.				29.180	29 94	35	
	9					29.180	29.94	36	
11	7	Fair, hard froſt.	ENE	20		29.184	29.96	30	
	12					29.168	29.88	35	
	9	Cloudy.		55		29.160	29.84	34	
12	7	Cloudy, hard froſt.	E	50		29.170	29.89	31	
	12	Fair.				29.186	29.97	33	
	9	Cloudy.				30. 0	30. 0	29	
13	7	Fair, very hard froſt.	WNW	10	NE	30. 4	30. 2	27	
	12	Cloudy.	NW			29.184	29.96	38	
	9		N		NNE	29.184	29.96	36	
14	7	Cloudy, froſt.	E	20		29.176	29.92	36	
	12	Thawing.				29.180	29.94	40	
	9					29.184	29.94	37	
15	7	Cloudy.	E	10		29.184	29.96	37	
	12	Sleet.				30. 0	30. 0	38	
	9	Fair, freezing.		60		30. 30	30.16	32	
16	7	Exceſſive froſt.	E	30		30. 40	30 21	24	
	12	Fair.				30. 40	30.21	28	
	9	Cloudy.				30. 40	30.21	30	
17	7	Cloudy and thawing.	ENE	15		30. 50	30.26	34	
	12					30. 50	30.26	36	
	9	Fair, freezing.				30. 50	30.26	32	
18	7	Fair, exceſſive froſt.	W	5	E	3. 64	30.33	21	
	12	Much milder.	S			30 64	30.33	37	
	9	Cloudy.	SE			30. 60	30.31	37	

February 1782.

Day.	Hour.	Weather.	Winds.		Clouds.	Barom. 192 parts.	Barom. 100 parts.	Thermom.	Rain.
						Inches.	Inches.	°	Inch.
19	7	Cloudy, a little froſt.	E	10		30. 60	30.31	35	
	12					30. 57	30.30	37	
	9					30. 48	30.25	37	
20	7	Cloudy.	W	10		30. 40	30.21	34	
	12					30. 40	30.21	39	
	9		WNW			29.180	29.94	37	
21	7	Fair.	S by W	0		29.160	29.84	37	
	12					29.144	29.75	41	
	9	Rain.				29. 32	29.17	40	
22	7	Fair.	W	30		29. 48	29.25	41	
	12					29. 40	29.21	44	
	9	Cloudy.				29. 32	29.17	40	
23	7	Hard rain and wind.	S	80		29. 0	29. 0	46	
	12					28.168	28.88	49	
	4					28.160	28.84		
	9		WNW	40		28.170	28.89	46	
24	7	Fair.	WNW	30		29. 30	29.16	45	0.67
	12					29.100	29.52	47	
	9	Rain in the night.				29.168	29.88	40	
25	7	Fair.	W by N	35		29.172	29.90	45	
	12					30. 12	30. 6	48	
	9					30. 0	30. 0	47	
26	7	Small ſhowers.	W	75		29.186	29.97	48	
	12	Fair and mild.				30. 0	30. 0	61	0.24
	9					30. 0	30. 0	57	
27	7	Cloudy.	S	5		29.180	29.94	50	
	12					29.180	29.94	52	
	9	Fair.				29.184	29.96	45	

February

February 1782.

Day.	Hour.	Weather.	Winds.		Clouds.	Barom. 192 parts.	Barom. 100 parts.	Thermom.	Rain.
						Inches.	Inches.	°	Inch.
	7	Fair.	W	25		30. 40	30.21	45	
28	12					30. 48	30.25	48	
	9					30. 32	30.17	44	
								Total rain	1.54

March 1782

Day.	Hour.	Weather.	Winds.	Clouds.	Barom. 192 parts.	Barom. 100 parts.	Thermom.	Rain.
					Inches.	Inches.	°	Inch.
1	6	Fair.	W 10		30. 0	30. 0	42	
	12				29.186	29.97	45	
	9		40		29.174	29.91	40	
2	6	Fair.	WNW 15		29.170	29.89	38	
	12				29.176	29.92	48	
	9	Cloudy.	NW		29.180	29 94	40	
3	6	Fair, white froſt.	NW 0		29.186	29.97	39	
	12				30. 0	30. 0	47	
	9		20		30. 16	30. 9	40	
4	6	Hard froſt.	W 0		30. 30	30.16	32	
	12				30. 16	30. 9	37	
	9		20		30. 16	30. 9	37	
5	6	Little froſt.	WSW 30		29 178	29.93	38	
	12				29 170	29.89	45	
	9				29.172	29.90	40	
6	6	Fair.	WNW 10		29.168	29.88	40	
	12				29.168	29.88	50	
	9				29.164	29.86	47	
7	6	Fair.	W 20		29.158	29.83	40	
	12				29.150	29.78	44	
	9	Showers.	75		29.144	29.75	48	
8	6	Sleet, but ſnow on the hills.	SSW 70		29.144	29.75	39	
	12		50		29 32	29.17	40	
	9	Very great ſtorm of wind and rain.	WNW		28.112	28.59		
	10		90		28.120	28 63	37	
9	6	Fair.	WbyN 40		29 48	29.25	39	0.82
	12	Showers.			29 60	29 31	45	
	9				29.132	29.69	4	

March 1782.

Day.	Hour.	Weather.	Winds.	Clouds.	Barom. 192 parts.	Barom. 100 parts.	Thermom.	Rain.
					Inches	Inches	°	Inch.
10	6	Showery day.	SE 20		29.144	29.75	43	
	12		SW 25		29.140	29.73	50	
	9		W		29.140	29.73	48	
11	6	Fair.	W 20		29.138	29.72	45	
	12		40		29. 96	29.50	53	
	9	Very ſtormy.	85		29. 48	29.25	48	
12	6	Stormy.	NW. 75		29. 92	29.48	42	
	12	Hail ſhowers.			29.140	29.73	45	
	9	Fair.	80		29 184	29.96	39	
13	6	Fair and froſt.	NE 20	W	30. 12	30. 6	34	
	12				30. 20	30.11	46	
	9		WSW 15		30. 10	30. 5	40	
14	6	Cloudy, froſt.	ENE 20		30. 8	30. 4	34	
	12	Fair.			30. 16	30. 9	44	
	9				30. 16	30. 9	37	
15	6	Fair, froſt.	WNW 25		30. 24	30.13	33	
	12				30. 26	30.14	40	
	9	Cloudy.			30. 20	30.11	40	
16	6	Fair,	NNE 30		30. 17	30. 9	39	
	12				30. 17	30. 9	41	
	9		W		30. 10	30. 5	40	
17	6	Cloudy.	NNW 35		30. 0	30. 0	38	0.10
	12	Fair.			29.188	29.98	45	
	9				29.190	29.99	40	
18	6	Hazy.	WNW 10		30. 0	30. 0.	37	
	12	Somewhat fair.			29.186	29.97	45	
	9	Cloudy.			29.180	29.94	41	

March 1782

Day.	Hour.	Weather.	Winds.	Clouds.	Barom. 192 parts.	Barom. 100 parts.	Thermom.	Rain.
					Inches.	Inches.	°	Inch.
19	6	Cloudy.	W 25		29.150	29.79	45	
	12	Some ſmall rain.			29 136	29.71	50	
	9				29.120	29.63	42	
20	6	Hazy.	SW 15		29.140	29.73	39	
	12	Fair.			29.152	29.80	45	
	9				29.144	29.75	41	
21	6	Sleet, but ſnow on the diſtant hills.	SE 45		29 128	29.67	37	
	12				29. 96	29.50	35	
	9				29. 40	29.21	35	0.40
22	6	Ditto.	E 60		28 160	28.84	33	
	12				28.170	28.89	34	
	9		ENE		29. 0	29. 0	32	0.50
23	6	Exceſſive froſt, with a little ſnow here, but five or ſix feet deep in the country.	E 10		29. 20	29.11	27	
	12		N		29. 32	29.17	35	
	9		NW 30		29. 88	29.46	37	
24	6	Some ſhowers.	NW 50		29.100	29.52	40	
	12				29. 90	29.47	43	
	9				29.100	29.52	40	0.33
25	6	Cloudy.	W 30		29.108	29.57	40	
	12	Fair.			29.112	29.59	47	
	9				29.128	29.67	39	
26	6	Showery.	WNW 25		29.144	29.75	45	
	12				29.140	29.73	50	
	9				29.140	29.73	48	
27	6	Showery.	W 32		29.128	29.67	48	
	12				29.120	29.63	51	0.17
	9	Hard rain all night.	SW 50		29. 88	29.46	48	

kept at Minehead, *in* Somerſetſhire.

March 1782.

Day.	Hour.	Weather.	Winds.	Clouds.	Barom. 192 parts.	Barom. 100 parts.	Thermom.	Rain.
					Inches.	Inches.	°	Inch.
28	6	Hard rain.	SSW 50		29. 32	29.17	51	
	12	Fair and mild.	30		29. 90	29.47	55	
	9	Cloudy.			29. 72	29.37	50	1.25
29	6	Hard rain.	WSW 60		29. 48	29.25	47	
	12	Fair.			29. 60	29 31	50	
	9				29. 78	29.41	49	
30	6	Fair.	WNW 50		29. 32	29.17	48	
	12				29. 64	29.34	55	
	9		NW		29. 92	29.48	50	
31	6	Very ſtormy.	W 60		29. 38	29.20	47	
	12	Showers.			29. 56	29.29	50	
	9		80		29. 46	29.24	46	0.34
							Total rain	3.91

April 1782.

Day.	Hour.	Weather.	Winds.	Clouds.	Barom. 192 parts.	Barom. 100 parts.	Therm.	Rain.
					Inches.	Inches.		Inch.
1	6	Hard rain ſhowers; the bar. fell till 8 in the evening: after which it roſe 1 line by 10; after which the wind roſe to a great ſtorm, but no rain.	SE 60		29. 0	29 0	44	
	12		SSW 50		28.116	28.61		
	8		W 30		28. 96	28.50	50	
	9				28. 44	28 23		
	10		WNW		28. 60	28 31	43	
2	6		NW 90		28 140	28.73	40	
	12				28.156	28.82	41	
	9		70		28.172	28.90	40	0.33
3	6	Some ſhowers.	NW 50		29. 0	29. 0	38	
	12	Fair.			29. 16	29. 9	45	
	9				29. 48	29.25	37	
4	6	Cloudy.	WNW 40		29. 76	29.40	40	
	12	Fair.			29. 80	29 46	45	
	9		10		29. 75	29.39	38	
5	6	Hard hail ſhowers.	E 40		29. 60	29.31	41	
	12	Fair.	NW 25	NEE	29. 72	29.38	41	
	9				29. 80	29.42	45	
6	6	Showers.	WNW 30		29. 96	29.50	40	
	12		E NW		29.108	29.57	44	
	9	Hail ſhowers.	NE 40		29.112	29.59	40	
7	6	Cloudy.	NE 50		29.128	29.67	40	
	12				29 156	29.82	45	
	9				30. 0	30. 0	41	
8	6	Cloudy.	NE 30		30. 0	30. 0	41	
	12	Fair.			30. 0	30. 0	45	
	9				30. 0	30. 0	40	
9	6	Cloudy.	NE 20		29.184	29 96	40	
	12	Fair.			29.186	29.97	44	
	9				29.186	29.97	40	

April 1782.

Day.	Hour.	Weather.	Winds.	Clouds.	Barom. 192 parts.	Barom. 100 parts.	Thermom.	Rain.
					Inches.	Inches.	°	Inch.
10	6	Cloudy.	NE 15		29.144	29.75	41	0.14
	12	Fair.			29.123	29.63	48	
	9	Cloudy.	WNW 20		29. 96	29.50	42	
11	6	Rain.	NW 30		29. 80	29.42	39	
	12	Cloudy.			29. 80	29.42	44	
	9				29. 62	29.32	38	
12	6	Fair, froſt.	WNW 20		29. 50	29.26	31	
	12	Rain, but ſnow on the hills.	ENE 15		29. 50	29.26	41	
	9				29. 44	29.23	40	
13	6	Cloudy.	NE 30		29. 72	29.38	40	
	12	Showers.			29. 80	29.42	41	
	9				29. 70	29.37	41	
14	6	Cloudy.	NE 10	NW	29. 80	29.42	38	
	12	Fair.			29. 92	29.48	44	
	9	Cloudy.			29. 96	29.50	40	
15	6	Cloudy.	NE 30		29. 90	29.47	40	
	12				29. 96	29.50	44	
	9	Showers.			29.104	29.54	40	
16	6	Cloudy.	NE 37		29.108	29.57	40	
	12	Showers.			29.100	29.52	45	
	9				29.100	29 52	40	
17	6	Rain.	ENE 30		29. 88	29.46	39	
	12				29. 90	29.47	42	
	9				29. 93	29.48	41	
18	6	Showers.	E 10		29. 96	29.50	40	
	12				29. 99	29.52	41	
	9				29.104	29.54	40	0.50

April 1782

Day.	Hour.	Weather.	Winds.	Clouds.	Barom. 192 parts.	Barom. 100 parts.	Thermom.	Rain.
					Inches.	Inches.	°	Inch.
19	6	Fair.	ESE 25		29.112	29.59	40	
	12				29.116	29.61	44	
	9				29.116	29.61	41	
20	6	Showery.	E 15		29.128	29.67	42	
	12				29.128	29.67	45	
	9				29.140	29.73	40	
21	6	Rain.	E 20		29.130	29.68	41	
	12		SW 35		29.120	29.63	46	
	9				29.128	29.67	41	0.10
22	6	Fair.	SW 40		29.115	29.60	50	
	12	A little rain.			29.126	29.66	56	
	9	Fair.	SE 45		29.123	29.64	46	
23	6	Fair.	SW 30		29.110	29.57	46	
	12	Showers.			29.115	29.60	55	
	9				29.107	29.56	49	
24	6	Showers.	SE 25		29. 78	29.41	50	
	12				29. 66	29.34	49	
	9	Hard rain.			29. 51	29.27	50	
25	6	Fair.	W 20		29. 73	29.38	49	0.67
	12		SE	W	29. 96	29.50	50	
	9				29. 96	29.50	49	
26	6	Fair.	E 30		29.112	29.59	48	
	12				29.116	29.61	50	
	9				29.126	29.66	47	
27	6	Cloudy, black eaſterly wind.	E 55		29.142	29.74	45	
	12				29.142	29.74	49	
	9				29.146	29.76	45	

April 1782.

Day.	Hour.	Weather.	Winds.	Clouds.	Barom. 192 parts.	Barom. 100 parts.	Thermom.	Rain.
					Inches.	Inches.	°	Inch.
28	6	Cloudy, black eaſterly wind.	E 50		29.140	29.73	41	
	12				29.140	29.73	46	
	9				29.136	29.71	42	
29	6	Rain all day, but ſnow on the hills.	E 45		29.138	29.72	40	
	12				29.144	29.75	38	
	9				29.160	29.84	40	
30	6	Cloudy, froſt.	SE 30		29.170	29.89	34	0.5
	12	Fair.			29.176	29.92	46	
	9				29.180	29.94	40	
							Total rain	1.79

May 1782

Day.	Hour.	Weather.	Winds.	Clouds.	Barom. 192 parts.	Barom. 100 parts.	Thermom.	Rain.
					Inches.	Inches.	°	Inch.
1	6	Fair, froſt.	ESE 28		29.180	29.94	34	
	12	No leaves on the trees yet.			29.182	29.95	47	
	9	Cloudy.			29.176	29 92	48	
2	6	Fair.	E 10		29.176	29.92	40	
	12	Cloudy.			29.170	29.89	48	
	9				29.166	29.87	45	
3	6	Cloudy.	E 15		29.152	29.80	41	
	12	Sometimes fair.			29.150	29.79	49	
	9	Cloudy.			29.146	29.77	46	
4	6	Fair.	E 12		29.185	29.70	47	
	12	Cloudy.			29.128	29.67	56	
	9		NE		29.116	29.61	45	
5	6	Cloudy.	NE 20		29.130	29.68	42	
	12		E		29.130	29.68	49	
	9		NW		29.126	29.66	45	
6	6	Cloudy.	NW 20	N	29.146	29.77	45	
	12				29.146	29.77	51	
	9				29.146	29.77	45	
7	6	Cloudy.	W 30	E	29.164	29.86	48	
	12		NE		29.165	29.86	51	
	9		W		29.173	29.90	47	
8	6	Fair.	SE 15		29.169	29.89	50	
	12	Showers.			29.160	29.84	54	
	9		S by E		29.128	29.67	48	
9	6	Cloudy.	SE by S 10		29. 96	29.50	50	
	12				29. 86	29.45	50	
	9				29. 94	29.49	47	

May 1782.

Day.	Hour.	Weather.	Winds.	Clouds.	Barom. 192 parts.	Barom. 100 parts.	Thermom.	Rain.
					Inches.	Inches.	°	Inch.
10	6	Cloudy.	ENE 30		29.104	29.54	47	
	12	Mild rain.	S		29.104	29.54	58	
	9	Showers.	WNW		29.107	29.56	51	
11	6	Showers.	SSW 25		29. 76	29.40	50	
	12				29. 70	29.37	54	
	9		WSW		29. 32	29.17	48	
12	6	Fair.	WSW 30		29. 80	29.42	47	
	12	Hard ſhowers.			29. 82	29.43	50	
	9	Fair.			29. 58	29.31	54	
13	6	Fair.	W 20		29.115	29.60	50	1.19
	12	Showers.			29.116	29.61	58	
	9		S		29.116	29.61	50	
14	6	Rain.	SSW 15		29. 75	29.39	56	
	12				29. 56	29.29	60	
	9	Cloudy.	WSW		29. 56	29.29	55	
15	6	Showers.	S by W 5		29. 66	29.34	55	
	12		SSW		29. 66	29.34	56	
	9		W by S		29. 58	29.30	53	
16	6	Showers.	W by S		29. 50	29.26	50	
	12				29. 70	29.36	59	
	9				29. 88	29.46	50	0.24
17	6	Fair.	W 15		29. 21	29.12	55	
	12				29. 15	29. 8	61	
	9	Rain.	SW by W		29. 35	29.18	53	
18	6	Showers.	WSW 15		29. 38	29.20	47	
	12	Flying clouds.			29. 79	29.42	54	
	9	Cloudy.			29. 90	29.47	49	

May 1782.

Day.	Hour.	Weather.	Winds.	Clouds.	Barom. 192 parts.	Barom. 100 parts.	Thermom.	Rain.
					Inches.	Inches.	°	Inch.
19	6	Rain.	NW 20		29.116	29.61	49	
	12	Fair.			29.134	29.70	51	
	9				29.164	29.86	45	
20	6	Fair.	S		29.161	29.84	49	
	12		SE by E 30		29.116	29.61	59	
	9	Hard rain.			29. 76	29.40	42	*
21	6	Rain.	NE 45		29. 88	29.46	46	
	12		WSW		29.104	29.54	49	
	9	Cloudy.			29.104	29.54	47	
22	6	Foggy, rain.	WNW 20		29.108	29.56	50	
	12	Fair.			29. 96	29.50	55	
	9				29. 90	29.47	48	
		Rain in the night.						
23	6	Showers.	SSE 40		29. 75	29.89	47	0.93
	12	Fair.			29. 90	29.47	50	
	9		WNW		29. 98	29.51	49	
24	6	Showers.	NW 50		29.112	29.59	50	
	12	Fair.	60		29.136	29.71	55	
	9				29.160	29.84	50	
25	6	Hazy.	SW 30		29.176	29.92	50	
	12	Fair.			29.170	29.89	54	
	9				29.146	29.77	50	
26	6	Fair.	W 30		29.154	29.81	50	0.24
	12				29.173	29.90	56	
	9	Rainy.			29.170	29.89	53	
27	6	Hazy.	SW 25		29.144	29.75	48	
	12	Showers.			29.144	29.75	62	
	9		S		29.140	29.73	55	

May 1782.

Day.	Hour.	Weather.	Winds.	Clouds.	Barom. 192 parts.	Barom. 100 parts.	Thermom.	Rain.
					Inches.	Inches.	°	Inch.
28	6	Cloudy.	SW 0		29.140	29.73	60	
	12	Rain.	SE 20		29 134	29.70	60	
	9	Hard rain.	SW		29.106	29.55	62	
29	6	Rain.	S 30		29. 96	29.50	60	
	12				29. 88	29.46	58	
	9	Cloudy.	W		29. 90	29.47	52	0.33
30	6	Small ſhowers.	W 25		29. 94	29.49	57	
	12	Fair at intervals.			29.106	29.55	62	
	9				29.106	29.55	55	
31	6	Cloudy.	W 0		29. 96	29.50	52	
	12	Hard ſhowers.			29. 96	29.50	60	
	9				29. 96	29.50	58	0.28
							Total rain	3.21

June 1782.

Day.	Hour.	Weather.	Winds.	Clouds.	Barom. 192 parts.	Barom. 100 parts.	Thermom.	Rain.
					Inches.	Inches.	°	Inch.
1	6	Rain.	NE 30	W	29.128	29.67	47	
	12	Cloudy.	NW by N		29.138	29.72	56	
	9	Rain.			29.124	29.65	49	
2	6	Fair.	NNW 40		29 188	29.98	50	
	12	Flying clouds.	NW		29 190	29 99	55	
	9				30. 4	30. 2	50	0.31
3	6	Fair.	NW 50		30. 8	30. 4	50	
	12				30. 16	30. 9	56	
	9	Cloudy.			30. 19	30.10	52	
4	6	Hazy.	WNW 20		30. 18	30.10	52	
	12	Fair.			30. 10	30. 5	60	
	9	Cloudy.			30. 0	30. 0	55	
5	6	Fair.	NW 30		30. 0	30. 0	50	
	12				30. 2	30. 1	58	
	9				30. 4	30. 2	52	
6	6	Fair.	E 10		30. 6	30. 3	47	
	12				30. 6	30. 3	60	
	9				30. 6	30. 3	52	
7	6	Fair.	SE 0		30. 5	30. 3	50	
	12	Hazy.			30. 0	30. 0	67	
	9	Cloudy.			29.184	29.96	60	
8	6	Cloudy.	SSW 10		29.163	29.86	55	
	12	Fair and hot.			29.152	29.80	70	
	9	Some rain.			29.138	29.72	57	
9	6	Showers.	S by W 25		29.100	29.52	56	
	12	Fair.			29.108	29.56	67	
	9	Cloudy.			29.112	29.59	57	0.50

June 1784.

Day.	Hour.	Weather.	Winds.		Clouds.	Barom. 192 parts.	Barom. 100 parts.	Thermom.	Rain.
						Inches.	Inches.	°	Inch.
10	6	Showers.	W	30		29. 62	29.31.	52	
	12	Fair.				29. 76	29.40	62	
	9	Showers.				29. 76	29.40	60	
11	6	Showers.	W	20		29. 57	29.30.	55	
	12	Fair.		50		29. 96	29.50	62	
	9					29.116	29.61	58	0.29
12	6	Showers.	W	20	SE	29. 56	29.29	55	
	12		SSE	30		29.112	29.59.	63	
	9		W	75		29. 76	29.40	55	
13	6	Showers.	W	50		29. 82	29.43	51	
	12	Fair.				29.116	29.61	62	
	9	Hard rain.				29.134	29.70	55	
14	6	Showers.	W by S	75		29.144	29.75	56	0.34
	12	Fair and hot.				29.150	29.79	69	
	9					29.160	29.84	60	
15	6	Cloudy.	WSW	20		29.184	29.96	61	
	12	Fair, very hot.		50		30. 0	30. 0	75	
	9			10		30. 4	30. 2	60	
16	6	Foggy.	SE	10	W	30. 16	30. 9	60	
	12	Fair.	NW	30		30. 32	30.17	72	
	9		SW			30. 24	30 18	60	
17	6	Fair.	ESE	20		29.188	29.98	60	
	12		S by E	10		29.164	29.86	71	
	4	Exceſſive hot, ther. roſe 21°.						81	
	9					29.150	29 79	75	
18	6	Flying thunder ſhowers.	NW	10		29.154	29.81	67	
	12	Fair.	WNW			29.172	29.90	67	
	9					30. 0	30. 0	58	

June 1782.

Day.	Hour.	Weather.	Winds.	Clouds.	Barom. 192 parts.	Barom. 100 parts.	Thermom.	Rain.
					Inches.	Inches.	°	Inch.
19	6	Fair.	WNW 10		30. 16	30. 9	58	
	12				30. 34	30.18	67	
	9				30. 32	30.17	60	
20	6	Fair.	ENE 10	W	30. 32	30.17	58	
	12		WSW		30. 38	30.20	68	
	9				30. 38	30.20	64	
21	6	Hazy.*	NNW 20		30. 44	30.23	60	
	12	Hot.	SW		30. 57	30.30	70	
	9				30. 76	30.40	67	
22	6	Fair and hot.	W 25		30. 76	30.40	60	
	12				30. 76	30.40	71	
	9		SE	SW	30. 76	30.40	60	
23	6	Fair, very hot.	ENE 30		30. 72	30.38	59	
	12				30. 61	30.32	74	
	9		ESE 40		30. 48	30.25	66	
24	6	Fair, very hot.	ESE 20		30. 30	30.16	62	
	12		S by E 10		30. 22	30.12	76	
	9				30. 19	30.10	67	
25	6	Fair.	W 0		30. 8	30. 4	67	
	12	Exceſſive hot.	E 22	SW	29.186	29 97	77	
	2						80	
	9		S		29.184	29.96	72	
26	6	Foggy and cooler.	WNW 25		29.184	29.96	61	
	12	Fair.			29.184	29.96	70	
	9				29.184	29 96	58	
27	6	Fair.	E 0		30. 10	30. 5	57	
	12		WNW 30		30. 10	30. 5	67	
	9				30. 0	30. 0	56	

June

June 1782.

Day.	Hour.	Weather.	Winds.	Clouds.	Barom. 192 parts.	Barom. 100 parts.	Thermom.	Rain.
					Inches.	Inches.	°	Inch.
28	6	Fair.	NW 10		29.188	29.98	57	
	12				30. 4	30. 2	67	
	9				30. 4	30. 2	60	
29	6	Cloudy.	W 20		30. 6	30. 3	56	
	12				30. 6	30. 3	67	
	9				30. 0	30. 0	56	
30	6	A little rain.	W 10		29.176	29.92	61	
	12	Hazy.	WNW		29.180	29.94	67	
	9	Fair.			29.180	29.94	55	

Total rain 1.44

July 1782.

Day.	Hour.	Weather.	Winds.	Clouds.	Barom. 192 parts.	Barom. 100 parts.	Thermom.	Rain.
					Inches.	Inches.	°	Inch.
1	6	Cloudy.	WNW 20		29.180	29.94	58	
	12				29.184	29.96	62	
	9	A hard ſhower.			29.172	29.90	57	
2	6	Flying clouds.	NW 40		29.144	29.75	58	
	12				29.146	29.76	62	
	9				29.152	29.79	57	0.07
3	6	Hazy.	WNW 15		29.182	29.95	59	
	12	Fair.			29.186	29.97	62	
	9				29.180	29.94	58	
4	6	A little rain.	WNW 30		29.162	29.85	58	
	12	Hazy.			29.166	29.87	62	
	9	Fair			29.170	29.89	55	
5	6	Fair and cold.	WNW 10		29.158	29.83	50	
	12	Rain.			29.140	29.73	61	
	9				29.130	29.68	58	0.28
6	6	Rain.	SE 20	SW	29.122	29.64	59	
	12		E 35		29.134	29.70	61	
	9		NE		29.150	29.79	60	0.23
7	6	Foggy, rain.	NE 20		29.162	29.85	59	
	12	Fair.			29.162	29.85	62	
	9	Cloudy.	0		29.162	29.85	60	
8	6	Cloudy.	E		29.167	29.88	60	
	12	Fair.	WNW 40		29.174	29.91	63	
	9	Cloudy.			29.174	29.91	58	
9	6	Fair and hot.	WNW 45		29.156	29.82	60	0.13
	12				29.160	29.84	70	
	9				29.170	29.89	59	

July

July 1782.

Day.	Hour.	Weather.	Winds.	Clouds.	Barom. 1000 parts.	Barom. 100 parts.	Thermom.	Rain.
					Inches.	Inches.	°	Inch.
10	6	Cloudy.	WNW 20		29.168	29.88	59	
	12	Fair.			29.170	29.89	60	
	9				29.175	29.91	58	
11	6	Rainy day.	SE 30	W by N	29 154	29.81	59	
	12		SSE		29.128	29.67	60	
	9		WNW		29.100	29.52	58	
12	6	A little shower.	W 0		29.122	29.64	56	
	12	Clouds and fair at intervals.			29.144	29.75	68	
	9	Hard showers.			29 152	29.80	58	
13	6	Rain.	S by E 30		29.164	29.80	59	
	12				29.132	29.69	61	
	9	Hard rain.			29.100	29.52	58	
14	6	Cloudy.	W 10		29.128	29.67	59	0.68
	12	Fair.			29.140	29.73	64	
	9				29.152	29.80	60	
15	6	Hazy.	W 20		29.164	29.86	60	
	12	Fair and hot.			29.174	29.91	74	
	9				29.160	29.84	65	
16	6	Fair.	E 0		29.154	29.81	59	
	12		NW 10		29.150	29 79	65	
	9	Thunder showers.	SE		29.144	29.70	60	0.40
17	6	Hard rain.	S by E 30		29.166	29.87	58	
	12				29.170	29.89	61	
	9		NW		29.186	29.97	59	
18	6	Fair and cold morning.	WNW 15		30. 0	30. 0	50	0.37
	12				30. 16	30. 9	69	
	9				30. 32	30.17	62	

July

July 1782.

Day.	Hour.	Weather.	Winds.	Clouds.	Barom. 192 parts.	Barom. 100 parts.	Thermom.	Rain.
					Inches.	Inches.	°	Inch.
19	6	Fair and hot.	W 10		30. 40	30.21	64	
	12		E	NE	30. 48	30.25	72	
	9				30. 36	30.19	65	
20	6	Fair.	WNW 15		30. 38	30.20	62	
	12		E	ENE	30. 30	30.16	67	
	9				30. 24	30.13	59	
21	6	Fair.	ENE 10		30. 24	30.13	64	
	12	Very hot.			30. 16	30. 9	74	
	9				30. 0	30. 0	68	
22	6	Fair.	E 30		29.160	29.84	61	
	12	Exceſſive hot.			29.152	29.80	80	
	2						81	
	9	Flying ſhowers.	S by E		29.140	29.73	68	
23	6	Flying ſhowers.	S 10		29.146	29.77	64	
	12				29.150	29.79	74	
	9	Fair.			29.154	29.81	65	
24	6	Cloudy.	SW 35	NE	29.158	29.83	63	
	12	Some ſmall rain.			29.166	29.87	71	
	9	Cloudy.	W	N	29.173	29.90	60	
25	6	Fair.	SW by W 40		29.188	29.98	59	
	12			ENE	29.190	29.99	65	
	9		W		30. 0	30. 0	60	
26	6	Cloudy.	SW 20	N	29.154	29.81	58	
	12	A hard ſhower.	WNW		29.162	29.85	64	
	9	Fair afternoon.	NE		29.172	29.90	59	
27	6	Rain.	NE 30	SW	29.156	29.82	61	
	12				29.150	29.79	63	
	9		NW		29.140	29.73	60	

July

July 1782.

Day.	Hour.	Weather.	Winds.	Clouds.	Barom. 192 parts.	Barom. 100 parts.	Thermom.	Rain.
					Inches.	Inches.	°	Inch.
28	6	Showers.	W 25		29.140	29.73	60	
	12				29.132	29.69	62	
	9	Cloudy.			29.138	29.72	60	
29	6	Fair.	E 10		29.150	29.79	60	
	12	Hazy.	W 35		29.154	29.81	70	
	9	A little rain.	NW		29.163	29.85	60	
30	6	Fair.	NE 20		29.170	29.89	56	0.77
	12			NE	29.172	29.90	65	
	9		WNW 40		29.174	29.91	58	
31	6	Cloudy.	WNW 15		29.160	29.84	56	
	12	Some rain.		SE	29.156	29.82	62	
	9		SW		29.144	29.75	60	
							Total rain	2.93

August

Auguſt 1781.

Day.	Hour.	Weather.	Winds.	Clouds.	Barom. 192 parts.	Barom. 100 parts.	Thermom.	Rain.
					Inches.	Inches.		Inch.
1	6	Flying ſhowers.	NE 30	S	29.140	29.73	58	
	12				29.128	29.67	68	
	9				29. 90	29.47	66	
2	6	Fair.	NE 0	S	29. 70	29.37	58	
	12	Showers.	WSW 30		29. 60	29.31	66	
	9	Showers.	WNW		29. 72	29.38	66	
3	6	Fair.	NW 40		29. 75	29.39	60	
	12		WNW		29.110	29.58	67	
	9				29.113	29.60	60	
4	6	Fair.	WNW 35		29.122	29.64	58	
	12				29.134	29.70	64	
	9	Showers.			29.144	29.75	60	
5	6	Some rain.	W 10		29.176	29.92	57	
	12	Fair.			29.168	29.88	65	
	9				29.160	29.84	59	0.19
6	6	Showers.	W		29.126	29.66	58	
	12	Fair.	SW		29. 96	29.50	68	
	9	Thunder ſhowers.	S		29. 48	29.25	60	0.11
7	6	Hard rain.	ESE 20		28.186	28.97	57	
	12	Very ſtormy.	NW 90		29. 19	29.10	51	
	9				29. 39	29.20	55	1.51
8	6	Very ſtormy ſhowers.	NNW 90		29. 56	29.29	56	
	12	Stormy, but fair.	80		29.100	29.52	64	
	9		75		29.125	29.65	60	0.30
9	6	Cloudy.	NW 40		29.120	29.63	58	
	12	Showers.			29.116	29.61	62	
	9				29.122	29.64	60	

Auguſt 1782.

Day.	Hour.	Weather.	Winds.	Clouds.	Barom. 192 parts.	Barom. 100 parts.	Thermom.	Rain.
					Inches.	Inches,		Inch.
10	6	Small ſhowers.	NW 50		29.135	29.71	58	
	12	Fair at intervals.			29.140	29.73	60	
	9				29.154	29.81	57	
11	6	Fair.	WNW 35		29.160	29.84	58	
	12				29.160	29.84	61	
	9		10		29.156	29.82	52	
12	6	Cloudy.	SE 30		29.134	29.70	58	
	12	Hard rain.			29.105	29.54	55	
	9				29 100	29.52	52	
13	6	Fair.	WSW 10		29.100	29.52	57	0.54
	12	Flying ſhowers.			29. 76	29.40	64	
	9	Hard wind and rain.	75		29. 36	29.19	60	
14	6	Hard ſhowers.	SW 75		29. 34	29.18	63	
	12	Fair.			29. 30	29.16	68	
	9	Cloudy.			29. 24	29.13	60	
15	6	Cloudy.	WNW 0		29. 80	29.42	62	
	12	Fair.			29.100	29.52	68	
	9				29.116	29.61	60	0.23
16	6	Rain.	S		29. 48	29.25	56	
	12	Fair.	WNW 20		29. 60	29.31	64	
	9	Cloudy.			29. 86	29 45	59	
17	6	Cloudy.	WNW 30		29. 86	29.45	58	
	12	Fair.			29. 86	29.45	60	
	9	Cloudy.			29. 86	29 45	59	
18	6	Fair.	NW 35		29.138	29.72	60	
	12		WNW		29.140	29.73	64	
	9	Showers.			29.144	29.75	60	

Auguſt 1782.

Day.	Hour.	Weather.	Winds.	Clouds.	Barom. 192 parts.	Barom. 100 parts.	Thermom.	Rain.
					Inches.	Inches.	°	Inch.
19	6	Fair.	SE 10		29.134	29.70	59	0.17
	12				29.146	29.76	69	
	9	Cloudy.	NW		29.148	29.77	60	
20	6	Cloudy.	SW 40		29.148	29.77	58	
	12	Showers.	WNW		29.144	29.75	70	
	9				29.144	29.75	60	
21	6	Fair.	WSW 25		29.152	29.79	58	
	12		SW		29.148	29.77	71	
	9	Showers.			29.154	29.80	63	
22	6	Showery day.	SW 40		29.144	29.75	58	
	12		WSW		29.134	29.70	67	
	9				29.122	29 64	57	
23	6	Showers.	W 50		29.126	29.66	56	
	12	Fair.	NW		29.138	29.73	67	
	9	Cloudy.	SW		29.154	29.81	60	
24	6	Cloudy.	SW 20		29.150	29.79	58	
	12	Showers.			29.115	29.60	70	
	9		WNW		29. 96	29.50	60	
25	6	Fair.	WSW 25		29.106	29.56	59	
	12				29.134	29.70	67	
	9	Showers.	W		29.150	29.79	60	
26	6	Showers:	WSW 20		29.154	29.81	60	
	12				29.163	29.85	70	
	9				29.173	29.90	61	0.64
27	6	Fair.	NW 45		29.138	29.72	60	
	12				29.135	29.70	64	
	9	Hard rain.	WNW		29.106	29.55	60	

Auguſt

August 1782.

Day.	Hour.	Weather.	Winds.	Clouds.	Barom. 192 parts.	Barom. 100 parts.	Thermom.	Rain.
					Inches.	Inches.		Inch.
28	6	Fair.	NW 50		29.122	29.64	58	
	12	Cloudy.			29.134	29.70	63	
	9	Showers.			29.130	29.68	58	
29	6	Showers.	W 40		29.111	29.58	52	
	12	Fair.	WNW		29.118	29.62	59	
	9	Hard rain.	65		29.123	29.64	54	
30	6	Showers.	NWbyW 45		29.142	29.74	55	
	12	Fair.			29.156	29.81	59	
	9				29.182	29.95	52	
31	6	Showers.	NWbyW 30		30. 4	30. 2	52	
	12	Fair.			30. 16	30. 9	60	
	9	Rain.			30. 24	30.13	55	0.50
							Total rain	4.19

September 1782.

Day.	Hour.	Weather.	Winds.	Clouds.	Barom. 192 parts.	Barom. 100 parts.	Thermom.	Rain.
					Inches.	Inches.	°	Inch.
1	6	Cloudy.	WSW 20		30. 32	30.17	57	
	12	Fair.	WNW		30. 37	30.20	60	
	9	Cloudy.			30. 40	30 21	54	0.17
2	6	Cloudy.	SW 10		30. 52	30.27	60	
	12	Fair.	NE NW		30. 66	30.34	68	
	9		SW		30. 48	30.25	58	
3	6	Cloudy.	SW 5		30. 29	30.15	52	
	12		NE SE	SW	30. 20	30.11	67	
	9	Fair	SW		30. 5	30. 3	60	
4	6	Fair.	NNE 10	SSE	29.180	29.94	55	
	12	Hot.	SSE		29.180	29.94	71	
	9				29.184	29.96	60	
5	6	Fair, cold rain.	SE 20		30. 0	30. 0	52	
	12	Hot.			30. 5	30. 3	70	
	9				30. 10	30. 5	60	
6	6	Fair.	SE 25		30. 10	30. 5	55	
	12				30. 15	30. 8	68	
	9				30. 15	30. 8	60	
7	6	Fair.	SSE 15		30. 18	30.10	60	
	12				30. 18	30.10	70	
	9				30. 20	30.11	60	
8	6	Fair.	E 10		30. 24	30.13	57	
	12				30. 24	30.13	67	
	9				30. 20	30 11	60	
9	6	Foggy.	E 10		30. 24	30.13	60	
	12	Fair.			30. 24	30.13	68	
	9				30. 14	30. 8	62	

September 1782.

Day.	Hour.	Weather.	Winds.		Clouds.	Barom. 192 parts.	Barom. 100 parts.	Thermom.	Rain.
						Inches.	Inches.	°	Inch.
10	6	Foggy.	E	15		30. 16	30. 9	58	
	12	Fair.				30. 10	30. 5	67	
	9					30. 0	30. 0	61	
11	6	Fair.	ENE	30		29 184	29.96	60	
	12					29.180	29.94	67	
	9			50		29.178	29.93	60	
12	6	Cloudy.	E	50		29.179	29.93	63	
	12					30. 0	30. 0	66	
	9	Fair.				30. 12	30. 6	60	
13	6	Cloudy.	E	20		30. 12	30. 6	55	
	12	Fair.				30. 0	30. 0	65	
	9		W			29.184	29.96	58	
14	6	Cloudy.	E	25		29.170	29.89	57	
	12					29 165	29.87	64	
	9					29.144	29.75	60	
15	6	A little rain.	S	25		29. 96	29.50	58	
	12	Fair.	ESE			29. 64	29.34	63	
	9					29. 48	29.25	60	
16	6	A little rain.	S	10		29. 20	29.11	60	
	12	Hard rain.	ESE	15		29. 12	29. 7	67	
	4					29. 0	29. 0		
	9	Stormy wind and rain.	SW	75		29. 33	29.17	57	
17	6	Fair.	W	25		29. 48	29.25	55	
	12	Showers.				29. 57	29.30	63	
	9					29. 57	29.30	56	0.61
18	6	Showers.	WNW	30		29. 48	29.25	55	
	12					29. 20	29.11	63	
	9			70		29. 57	29.30	58	

September 1782.

Day.	Hour.	Weather.	Winds.	Clouds.	Barom. 192 parts.	Barom 100 parts.	Thermom.	Rain.
					Inches.	Inches.	°	Inch.
19	6	Fair and cold.	WNW 60		29.130	29.68	51	
	12				29.170	29.89	53	
	9				29.176	29.92	52	
20	6	Showers.	WNW 30		29.150	29.79	60	
	12				29.142	29.74	64	
	9				29.110	29.58	60	0.22
21	6	Cloudy.	SW 25		29.100	29.52	58	
	12	Hard rain.			29. 96	29.50	66	
	9				29. 96	29.50	61	
22	6	Fair.	SW 28		29.100	29.52	60	
	12	Hard ſhowers.			29.100	29.52	67	
	9				29. 90	29.47	61	0.24
23	6	Showers.	NW 60		29. 76	29.40	57	
	12				29. 80	29.42	64	
	9	Stormy.	70		29.140	29.73	60	
24	6	A little ſhower.	WNW 10		29.164	29.86	53	
	12	Cloudy.			29.180	29.94	63	
	9	Rain.			29.170	29.89	58	0.88
25	6	Showers.	WNW 5		29.156	29.82	53	
	12	Fair and hot.			29.164	29.86	69	
	9				29.164	29 86	60	
26	6	Showers.	W 0		29.160	29.84	55	
	12	Fair and hot.			29.160	29 84	70	
	9	Cloudy.	20		29.160	29.84	58	0.45
27	6	Rain.	NW 30		29.125	29.65	55	
	12				29.150	29.79	67	
	9	Fair.			29.182	29.95	60	

September 1782.

Day.	Hour.	Weather.	Winds.	Clouds.	Barom. 192 parts.	Barom. 100 parts.	Thermom.	Rain.
					Inches.	Inches.	°	Inch.
28	6	Cloudy and cold.	SW 10		30. 6	30 3	49	
	12		35		30. 12	30. 6	57	
	9	Rain.			29.180	29.94	55	
29	6	Fair.	W 20		29.170	29.89	60	
	12	Showers.			29.140	29.73	67	
	9	Hard rain.			29.128	29.67	61	
30	6	Fair.	NW 60		29 116	29.61	50	
	12	Showers.			29.144	29.75	55	
	9				29.180	29.94	51	1.33
							Total rain	3.40

October 1782.

Day.	Hour.	Weather.	Winds.		Clouds.	Barom. 192 parts.	Barom. 100 parts.	Thermom.	Rain.
						Inches.	Inches:	°	Inch.
1	7	Fair.	NW	30		30. 8	30. 4	49	
	12					30. 10	30. 5	54	
	9					30. 0	30. 0	50	
2	7	Showers.	WNW	35		29. 80	29.42	55	
	12					29. 70	29.37	64	
	9					29. 48	29.25	57	0.41
3	7	Fair.	NE	15		29. 30	29.16	58	
	12	Cloudy.				29. 60	29.31	63	
	9					29. 80	29.42	56	
4	7	Fair and cold.	NE	20		29. 88	29.45	43	
	12					29. 90	29.47	53	
	9	Cloudy.				30. 12	30. 6	50	
5	7	Fair.	ENE	10		29.172	29.89	45	
	12					29 180	29.94	48	
	9					29.186	29.97	48	
6	7	Fair.	E	25		30. 6	30. 3	50	
	12					30. 0	30. 0	56	
	9	Cloudy.				29.184	29.96	51	
7	7	Cloudy.	WNW	10		29.175	29.91	50	
	12	A little rain.				29.175	29.91	55	
	9					29.170	29.89	51	
8	7	Moiſt cloſe day.	NW	30		29.164	29.86	53	
	12					29.160	29.84	56	
	9					29.160	29.84	51	
9	7	Some rain.	ESE	24		29.154	29.81	53	
	12					29.140	29.73	57	
	9					29.136	29.71	54	

October 1782.

Day.	Hour.	Weather.	Winds.	Clouds.	Barom. 192 parts.	Barom. 100 parts.	Thermom.	Rain.
					Inches.	Inches.	°	Inch.
10	7	Cloudy.	E 30		29. 80	29.42	50	
	12				29. 40	29.21	56	
	9		60		29. 16	29. 9	52	
11	7	Cloudy.	E 60		29. 16	29. 9	49	
	12				29. 40	29.21	54	
	9				29. 60	29.31	51	
12	7	Cloudy.	E 50		29. 84	29.44	50	
	12				29. 88	29.46	54	
	9				29.110	29.57	51	
13	7	Fair.	E 20		29.130	29.68	48	
	12				29.154	29.81	52	
	9	Cloudy.			29.162	29.85	50	
14	7	White froſt.	W 10		29.184	29.96	37	
	12				30. 0	30. 0	47	
	9		E		30. 6	30. 3	50	
15	7	Some rain.	E 20		30. 16	30. 9	50	
	12				30. 16	30. 9	54	
	9				30. 16	30. 9	51	
16	7	Cloudy.	WNW 30		30. 16	30. 9	47	
	12	Fair.			30. 18	30.10	55	
	9				30. 20	30.11	50	
17	7	Fair.	N 35		30. 24	30.13	49	
	12		NW		30. 28	30.15	55	
	9	Cloudy.			30. 36	30.19	51	
18	7	Cloudy.	WNW 50		30. 58	30.31	50	
	12	Fair.			30. 72	30.38	54	
	9	Cloudy.	70		30. 66	30.34	50	

October 1782.

Day.	Hour.	Weather.	Winds.	Clouds.	Barom. 192 parts.	Barom. 100 parts.	Thermom.	Rain.
					Inches.	Inches.	°	Inch.
19	7	Some little rain.	WNW 60		29.154	29.81	52	
	12				29.160	29.84	56	
	9	Stormy wind.	80		29.165	29.86	50	0.42
20	7	Fair.	NW by N 60		29.170	29.89	47	
	12				29.172	29.90	54	
	9	Rain.			29.170	29.89	55	
21	7	Rain.	NW 40		29.124	29.65	56	
	12	Fair.			29.144	29.75	60	
	9	Cloudy.			29.144	29.75	60	
22	7	Cloudy.	WNW 35		29.144	29.75	60	
	12				29.130	29.68	62	
	9	Rainy night.			29.110	29.58	62	
23	7	Fair.	WNW 20		29.144	29.75	48	
	12				29.160	29.84	53	
	9				30. 15	30. 8	50	
24	7	Fair, froſt.	WNW 0		30. 38	30.20	39	
	12				30. 48	30.25	52	
	9		35		30. 58	30.30	50	
25	7	Showers.	W 20		30. 40	30.21	47	
	12				30. 32	30.17	54	
	9				30. 19	30.10	51	0.40
26	7	Fair.	W 0		30. 16	30. 9	55	
	12				30. 40	30.21	60	
	9	Foggy.			30. 60	30.31	55	
27	7	Foggy day.	SW 0		30. 56	30.29	54	
	12				30. 48	30.25	58	
	9				30. 32	30.17	57	

October 1782.

Day.	Hour.	Weather.	Winds.	Clouds.	Barom. 192 parts.	Barom. 100 parts.	Thermom.	Rain.
					Inches.	Inches.	°	Inch.
28	7	Fair.	W 5		29.184	29 96	50	
	12	Small ſhowers.	25		29 176	29.92	55	
	9	Stormy ſhowers.	75		29.104	29.54	54	
29	7	Exceſſive ſtormy, but dry.	WNW 90		29.136	29.72	50	
	12		NW 80		29.160	29.84	55	
	9	Shower.	10		30. 8	30. 4	51	
30	7	Rain.	WNW 0		30. 0	30. 0	40	
	12				29.140	29.73	52	
	9		30		29. 96	29.50	50	
31	7	Showers.	NW 25		29. 68	29.36	48	
	12				29. 79	29.42	50	
	9				30. 0	30. 0	46	0.46
						Total rain		1.69

November 1782.

Day.	Hour.	Weather.	Winds.	Clouds	Barom. 192 parts.	Barom. 100 parts.	Thermom.	Rain.
					Inches.	Inches.		Inch.
1	8	Cloudy.	SE 15		30. 0	30. 0	42	
	12	Showers.			29. 97	29.50	49	
	9				30. 6	30. 3	45	
2	8	Rain.	SE. 30		29.170	29.89	45	
	12				29. 84	29.44	50	
	9				29. 76	29 40	43	
3	8	Fair.	NW 10		29. 6	29. 3	45	
	12	Showers.	W		29. 12	29. 6	49	
	9		NE		29. 24	29.13	49	
4	8	Fair.	NW 30		29. 84	29.44	40	0.73
	12				29. 90	29.47	45	
	9		NE		29. 96	29.50	44	
5	8	Rain.	NE 35		29.104	29.54	39	
	12		ENE		29.136	29.71	43	
	9		NE		29.136	29.71	43	
6	8	Fair, froſt.	NW 23		30. 0	30. 0	38	
	12	Snow on the hills.			30. 12	30. 6	43	
	9				30. 50	30.26	37	
7	8	Fair, hard froſt.	NNW 0		30. 56	30.30	32	
	12		NE	E	30. 64	30.33	38	
	9		NW		30. 56	30.30	32	
8	8	Fair, hard froſt.	NE 20		30. 32	30.17	30	
	12		ENE		30. 24	30.13	40	
	9	Cloudy.			30. 16	30. 9	38	
9	8	Cloudy, little froſt.	NW 20		30. 0	30. 0	37	
	12	Froſt.			30. 0	30. 0	43	
	9				29.178	29.93	32	

November 1782.

Day.	Hour.	Weather.	Winds.	Clouds.	Barom. 192 parts.	Barom. 100 parts.	Thermom.	Rain.
					Inches.	Inches.	o	Inch.
10	8	Very hard froſt.	NW 0		29.170	29 89	27	
	12				29.166	29.87	35	
	9				29.164	29.86	38	
11	8	Rain.	ENE 25		29 144	29.75	37	
	12				29 160	29.84	42	
	9				29.172	29.90	40	
12	8	Foggy rain.	SSE 30		30. 0	30. 0	39	
	12				30. 8	30. 4	45	
	9	Fair.			30. 20	30.11	48	0.12
13	8	Fair, froſt.	NW 0		30. 80	30.42	32	
	12	N. B. The tide ebbed and	SE		30. 96	30.50	45	
	9	flowed 3 times in an hour.	W		30 120	30.63	35	
14	8	Fair, froſt.	W 0		30.114	30.60	35	
	12				30.112	30.59	40	
	9	Cloudy.			30.110	30.58	40	
15	8	Cloudy.	NW 10		30.108	30.57	48	
	12				30. 90	30.47	53	
	9	Rain.	50		30. 48	30.25	50	
16	8	Cloudy.	NW 75		30. 0	30. 0	48	
	12	Fair.			30. 0	30. 0	52	
	9				30. 16	30. 9	45	
17	8	Fair, little froſt.	NNW 30		30. 38	30.20	42	
	12				30. 32	30.17	45	
	9				30. 30	30.16	40	
18	8	Cloudy.	W 10		29.176	29.92	44	
	12				29.174	29.91	48	
	9	Small rain.			29.172	29.90	40	

November 1782.

Day.	Hour.	Weather.	Winds.	Clouds.	Barom. 192 parts.	Barom. 100 parts.	Thermom.	Rain.
					Inches.	Inches.	°	Inch.
19	8	Foggy, rain.	NW 20		29.180	29.94	40	
	12				29.180	29.94	47	
	9				29.180	29.94	43	
20	8	Cloudy.	E 15		29.180	29.94	40	0.10
	12	Fair.			29.180	29.94	47	
	9				29.180	29.94	41	
21	8	Fair, hard froſt.	WNW 0		29.180	29.94	31	
	12				30. 8	30. 4	37	
	9				30. 0	30. 0	30	
22	8	Very hard froſt.	S 0		29.154	29.81	29	
	12		SE		29.128	29.67	39	
	9	Cloudy.	W		29.104	29.54	38	
23	8	Cloudy.	NW 10		29. 80	29.42	38	
	12				29. 72	29.38	40	
	9	Snow.	SE		29. 70	29.37	36	
24	8	Cloudy.	S by E 15		29. 66	29.34	35	
	12		E		29. 56	29.29	39	
	9	Rain.			29. 62	29.32	38	
25	8	Hard rain.	S by E 10		29. 90	29.47	37	
	12				29. 96	29.50	38	
	9	Cloudy.			29.130	29.68	38	
26	8	Fair.	S		29.180	29.94	37	
	12				30. 0	30. 0	40	
	9	Freezing hard.	S by E 20		30. 8	30 4	32	
27	8	Cloudy.	S by E 20		29.160	29.84	38	
	12	Rain.	SW		29.152	29.80	40	
	9				29.144	29.75	40	

November 1782.

Day.	Hour.	Weather.	Winds.	Clouds.	Barom. 192 parts.	Barom. 100 parts.	Thermom.	Rain.
					Inches.	Inches.	°	Inch.
	8	Rain.	S by E 30		29.136	29.71	39	
28	12		SSW		29.130	29.68	42	
	9	Cloudy.			29.112	29.59	42	0.54
	8	Hail ſhowers.	NW 70		29.134	29.70	40	
29	12				29.134	29.70	43	
	9		E		29.134	29.70	42	
	8	Rain.	E 25		29.120	29.63	40	
30	12	Fair.			29.126	29.66	43	
	9				29.130	29.68	40	
						Total rain		1.4

December 1782.

Day.	Hour.	Weather.	Winds.	Clouds.	Barom. 192 parts.	Barom. 100 parts	Thermom.	Rain.
					Inches.	Inches.	°	Inch.
1	8	Cloudy.	ENE 10		29.154	29.81	37	
	12				29.160	29.84	42	
	9	Fair.			29.173	29.90	39	
2	8	Exceſſive hard froſt.	W 0		29.153	29.80	26	
	12		SE	W	29.153	29.80	35	
	9		ESE		29.153	29.80	30	
3	8	Fair, froſt.	SSE 10		30. 0	30. 0	32	
	12				30. 0	30. 0	35	
	9	Cloudy.			30. 0	30. 0	32	
4	8	Cloudy, froſt.	E by S 15		30. 20	30.11	32	
	12				30. 20	30.11	34	
	9				30. 20	30.11	32	
5	8	Cloudy, froſt.	E by S 25		29.176	29.92	34	
	12				29.168	29.88	34	
	9				29.154	29.81	34	
6	8	Cloudy, froſt.	S by W 10		29.134	29.70	40	
	12				29.126	29.66	45	
	9	Miſling rain.	S		29.126	29.66	42	
7	8	Cloudy.	S by W 12		29.140	29.73	41	
	12	Some rain.			29.140	29.73	49	
	9				29.152	29.80	47	0.14
8	8	Foggy and cold.	E 15		30. 0	30. 0	35	
	12				30. 8	30. 4	35	
	9				30. 8	30. 4	35	
9	8	Foggy.	E 20		29.154	29.81	35	
	12				29.150	29.79	37	
	9				29.150	29.79	37	

December 1782.

Day.	Hour.	Weather.	Winds.	Clouds.	Barom. 192 parts.	Barom. 100 parts.	Thermom.	Rain.
					Inches.	Inches.	°	Inch.
10	8	Foggy.	NW by W 5		29.140	29.73	40	
	12		NE		29.136	29.71	43	
	9				29.140	29.73	41	
11	8	Foggy.	E 15		29.190	29.99	36	
	12		S by E		29.190	29.99	38	
	9				29.188	29.98	37	
12	8	Foggy.	WNW 25		29.156	29.82	36	
	12				29.132	29.69	37	
	9	Rain.			29.128	29.67	37	
13	8	Rain.	WNW 20		29.112	29.59	40	
	12	Fair.			29.112	29.59	42	
	9	Hard rain.			29. 64	29.33	40	
14	8	Rain.	WNW 30		29. 96	29.50	39	
	12				29.108	29.57	41	
	9	Cloudy.			29.108	29.57	40	
15	8	Cloudy, ſometimes fair.	NW 35		29.122	29.64	39	
	12				29.124	29.65	42	
	9	Cloudy.			29.140	29.73	40	
16	8	Rain.	WNW 20		29.116	29.61	41	
	12				29.110	29.58	47	
	9	Very hard rain.	60		29.110	29.58	46	
17	8	Foggy, rain.	NW 40		29.112	29.59	50	
	12	Very mild.			29.118	29.62	54	
	9				29.116	29.61	51	1.09
18	8	Cloudy.	WNW 25		29.128	29.67	48	
	12	Fair.			29.160	29.84	50	
	9.				29.190	29.99	50	

December, 1784.

Days.	Hour.	Weather.	Winds.	Clouds.	Barom. 192 parts.	Barom. 200 parts.	Thermom.	Rain.
					Inches.	Inches.	°	Inch.
19	8	Frosty and fair.	W 0		30. 92	30.48	40	
	12		10		30 116	30.61	45	
	9	Hazy.			30.128	30.67	41	
20	8	Foggy.	W 15		30.120	30.63	39	
	12				30.120	30.63	42	
	9				30.116	30 61	40	
21	8	Cloudy.	WNW 20		30.116	30.61	39	
	12	Fair.			30.120	30.63	48	
	9		NW		30.116	30.61	41	
22	8	Frosty and fair.	E 0		30.112	30.59	39	
	12				30.106	30.55	44	
	9	Cloudy.			30.106	30.55	46	
23	8	Fair.	SW 20		30. 72	30.38	40	
	12	Cloudy.	WNW		30. 70	30.37	45	
	9				30. 57	30.30	40	
24	8	Cloudy.	NW 75		30. 50	30.26	45	
	12				30. 50	30.26	48	
	9				30. 50	30.26	45	
25	8	Foggy.	NW 25		30. 28	30.15	45	
	12				30. 40	30.21	49	
	9				30. 64	30.33	40	
26	8	Foggy.	WNW 15		30. 96	30.50	44	
	12				30. 96	30.50	48	
	9				30. 96	30.50	40	
27	8	Foggy.	WNW 25		30. 64	30.34	41	
	12				30. 60	30.32	45	
	9				30. 60	30.32	39	

December 1782.

Day.	Hour.	Weather.	Winds.	Clouds.	Barom. 192 parts.	Barom. 100 parts.	Thermom.	Rain.
					Inches.	Inches.	°	Inch.
28	8	Foggy.	NW 40		30. 44	30.23	38	
	12				30. 40	30.21	43	
	9	Miſling rain.			30. 30	30.16	40	
29	8	Miſling rain.	W by N 15		30. 30	30.16	40	
	12				30. 28	30.15	44	
	9	Fair.			30. 32	30.17	39	
30	8	Foggy.	NW 40		30. 40	30.21	39	
	12				30. 40	30.21	42	
	9				30. 44	30.23	40	
31	8	Fair.	ENE. 30		30. 50	30 26	40	
	12				30. 52	30 27	44	
	9		ESE 10		30. 54	30.28	38	
							Total rain	1.23

Total of Rain from the firſt of January, 31.26 inches.

IX. *Deſcription of a Meteor, obſerved* Aug. 18, 1783. *By Mr.* Tiberius Cavallo, *F. R. S.*

Read Jan. 15, 1784.

BEING upon the Caſtle Terrace at Windſor, in company with my friend Dr. JAMES LIND, Dr. LOCKMAN, Mr. T. SANDBY, and a few other perſons, we obſerved a very extraordinary meteor in the ſky, ſuch as none of us remembered to have ſeen before. We ſtood upon the north-eaſt corner of the terrace, where we had a perfect view of the whole phænomenon; and as every one of the company remarked ſome particular circumſtance, the collection of all which furniſhed the materials for this account, it may be preſumed, that this deſcription is as true as the nature of the ſubject can admit of.

The weather was calm, agreeably warm, and the ſky was ſerene, excepting very near the horizon, where an hazineſs juſt prevented the appearance of the ſtars. A narrow, ragged, and oblong cloud ſtood on the north-weſt ſide of the heavens, reaching from the extremity of the hazineſs, which roſe as high as 18 or 20 degrees, and ſtretching itſelf for ſeveral degrees towards the eaſt, in a direction nearly parallel to the horizon. It was a little below this cloud, and conſequently in the hazy part of the atmoſphere, about the N. by W. ½ W. point of the compaſs,

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compaſs, that this luminous meteor was firſt perceived. Some flaſhes of lambent light, much like the *aurora borealis*, were firſt obſerved on the northern part of the heavens, which were ſoon perceived to proceed from a roundiſh luminous body, nearly as big as the ſemidiameter of the moon, and almoſt ſtationary in the abovementioned point of the heavens (ſee A in the annexed figure, tab. IV). It was then about 25 minutes after nine o'clock in the evening *. This ball, at the beginning, appeared of a faint bluiſh light, perhaps from its being juſt kindled, or from its appearing through the hazineſs; but it gradually increaſed its light, and ſoon began to move, at firſt aſcending above the horizon in an oblique direction towards the eaſt. Its courſe in this direction was very ſhort, perhaps of five or ſix degrees; after which it turned itſelf towards the eaſt, and moving in a direction nearly parallel to the horizon, reached as far as the S. E. by E. where it finally diſappeared. The whole duration of the meteor was half a minute, or rather leſs; and the altitude of its track ſeemed to be about 25 degrees above the horizon. A ſhort time after the beginning of its motion, the luminous body paſſed behind the above-mentioned ſmall cloud, ſo that during this paſſage we obſerved only the light that was caſt in the heavens from behind the cloud, without actually ſeeing the body from which it proceeded, for about the ſixth or at moſt the fifth part of its track; but as ſoon as the meteor emerged from behind the cloud, its light was prodigious. Every object appeared very diſtinct; the whole face of the country in that beautiful proſpect before the terrace being inſtantly illuminated. At this moment the body of the meteor appeared of an oblong form, like that repreſented at B in the figure; but it preſently acquired a tail, and ſoon after it parted into ſeveral ſmall bodies, each having a tail, and all moving in the ſame direction, at a ſmall diſtance from each other, and very little behind the principal body, the ſize of which was gradually reduced after the diviſion (ſee D in the figure). In this form the whole meteor moved as far as the S. E. by E. where the light decreaſing rather abruptly, the whole diſappeared.

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* Mr. SANDBY's watch was ſeventeen minutes paſt nine neareſt; it does not mark ſeconds.

During the phænomenon no noiſe was heard by any of our company, excepting one perſon, who imagined to have heard a crackling noiſe, ſomething like that which is produced by ſmall wood when burning. But about ten minutes after the diſappearance of the meteor, and when we were juſt going to retire from the terrace, we heard a rumbling noiſe, as if it were of thunder at a great diſtance, which, to all probability, was the report of the meteor's exploſion; and it may be naturally imagined that this exploſion happened when the meteor parted into ſmall bodies, *viz.* at about the middle of its track.

Now if that noiſe was really the report of the exploſion which happened in the abovementioned place, the diſtance, altitude, courſe, and other particulars relating to this meteor, muſt be very nearly as expreſſed in the following liſt; they being calculated with mathematical accuracy, upon the preceding particulars; and upon the ſuppoſition that ſound travels 1150 feet per ſecond. But if the noiſe we heard was not that of the meteor's exploſion, then the following calculations muſt be conſidered as quite uſeleſs and erroneous.

Diſtance of the meteor from Windſor Caſtle	130 miles.
Length of the path it deſcribed in the heavens	550 miles.
Diameter of the luminous body when it came out of the clouds	1070 yards.
Its height above the ſurface of the Earth	56½ miles.

The exploſion muſt have happened perpendicularly over Lincolnſhire.

T. CAVALLO.

X. *An Account of the Meteors of the 18th of* Auguſt *and 4th of* October, 1783. *By* Alex. Aubert, *Eſq. F. R. S. and S. A.*

Read Jan. 15, 1784.

HAVING been fortunate enough to ſee both the Meteors, of the 18th of Auguſt and of the 4th of October laſt, I think it my duty to communicate the obſervations I made upon them to the Royal Society. We are in general ſo little acquainted with theſe phænomena, that too many accounts of them cannot be collected, in order to enable us to form ſome idea of their nature, path, magnitude, and diſtance from the earth. It is not to be expected, that an obſerver, in the open air, to whom the appearance comes totally unexpected, can give a perfect account of it; but by going afterwards to the ſpot from which he ſaw it, he may, by the aſſiſtance of the objects about him, and ſome proper inſtruments, come near the truth: I have followed this method; and it is the reſult thence deduced I have the honour of communicating to the Society.

Monday the 18th of Auguſt had been a very ſultry day. At the time the meteor made its appearance, although the ſtars were bright in the upper part of the heavens, the horizon was ſurrounded with a hazineſs which did not permit any ſtars to be ſeen under an altitude of about eight degrees. I was on horſeback, returning to my Obſervatory at Loampit-hill, near Deptford, in Kent; my face was turned towards the South

Weſt. I was at the foot of Lewiſham-bridge, when I was much ſurpriſed at perceiving ſuddenly a kind of glimmering light, reſembling faint but quickly repeated flaſhes of lightning; ſoon after which the light increaſed much towards the North Weſt; I turned directly to it, and ſaw it form into a large luminous body like electrical fire, with a tinge of blue round its edges. It roſe from the hazy part of the atmoſphere (which I have obſerved might be about 8° high), and moved at firſt almoſt in a vertical direction, changing its ſize and figure continually, having to me all the appearances of ſucceſſive inflammation, and not of a ſolid body; it was ſometimes round, at others oval and oblong, with its longeſt diameter in the line of its motion; although it had got high enough to be quite out of the hazy part of the horizon, it was ſurrounded and accompanied in its whole courſe with a kind of whitiſh miſt or light vapour. The place from which it roſe was about 38° from the north towards the weſt. After riſing a little way perpendicularly, it made its progreſs in a curve, ſo as to be at the higheſt when it had reached due eaſt, at an altitude of about 35°; after which, continuing a few degrees beyond the eaſt, and being about 30° high, it left behind it ſeveral globules of various ſhapes; the firſt which detached itſelf being very ſmall, and the others gradually larger and larger, until the laſt was nearly as large as the remaining preceding body; ſoon afterwards they all extinguiſhed gradually, like the bright ſtars of a ſky-rocket, with ſome inclination downwards, which appearance might probably ariſe from the upper parts of the ſeparate bodies extinguiſhing before the lower ones. The meteor was at the brighteſt and at the largeſt juſt before its ſeparation; I eſtimated its magnitude or area then to be equivalent to two full moons. Its light, during its whole courſe, was ſo great, that I could ſee every object diſtinctly, and when it was extinguiſhed the night appeared very dark: I could however ſee by my watch that it was ſeventeen minutes after nine: as ſoon as I got to my obſervatory, which might be about ten minutes afterwards, having compared it with my regulator, I found it about half a minute too ſlow for mean time. I think the whole appearance of the meteor, from its firſt riſing out of the hazy part of the atmoſphere to its total extinction, did not exceed ten or twelve ſeconds of time, during which it moved a ſpace correſponding to about 136° in azimuth. I recollect an appearance during its motion, which confirms me in the idea I had of its not being a ſolid body. In its progreſs it did not deſcribe a curve as regular as might have been expected from ſuch a body; but ſeemed to move in ſomewhat of a waving line. This irregularity in its courſe was probably owing to changes of its figure and ſize, occaſioned by the train of inflammation not running in an even line. I ſhould alſo mention that the meteor appeared extremely near to me, more particularly when it was at the higheſt; yet from the compariſons made already of obſervations at ſeveral diſtant places, we may reaſonably judge, that it could not be at leſs than 40 or 50 miles diſtance from the ſurface of the earth.

The meteor of Saturday the 4th of October laſt was of a much ſhorter duration and path. I was on horſeback, near the ſtones end, in Blackman-Street, Southwark; my face was turned northward. I ſaw, towards the N. N. E. a train of fire, reſembling in its motion a common meteor, vulgarly called a falling ſtar, but the colour of it was red; it originated at an altitude of about 25°, and moved quickly in a ſtrait line eaſtward, inclining gradually towards the horizon, ſo as to be, after a courſe of 15° or 20° in azimuth, about 15° above the horizon, when it ſpread into a broader train, and growing of a lighter colour, it terminated by reſolving itſelf into a beautiful oblong body of the brighteſt fire, like electrical fire tinged blue, almoſt as large as the moon; it illuminated the ſtreet and houſes much more than any lightning I have ſeen; thoſe who had not a direct view of it, took it for a long flaſh of lightning. I think its whole courſe did not exceed 25°, nor the time of its appearance two or three ſeconds. It extinguiſhed quickly, and left behind it, in its path, a train of very dull reddiſh fire, which continued viſible to my naked eye above one minute and a half. The time of night was forty-three minutes paſt ſix; it was a fine ſtar-light evening, warmer than the preceding ones; the moon beyond the firſt quarter, and very bright; yet her light was not to be compared to the much greater light of the meteor.

I do not recollect hearing any noiſe or report, either during or after the appearance of theſe meteors.

London,
Nov. 6, 1783.

ALEXANDER AUBERT.

Since I wrote the above account, I have reaſon to think I have eſtimated the altitude of the laſt meteor rather too low; ſome of my friends in London, who had, at the time of its appearance, a very good object of compariſon for its altitude, make it nearer 30 than 20 degrees.

XI. *Obſervations on a remarkable Meteor ſeen on the* 18*th of* Auguſt, 1783, *communicated in a Letter to Sir* Joſeph Banks, *Bart. P. R. S. By* William Cooper, *D. D. F. R. S. Archdeacon of* York.

Read Jan. 15, 1784.

DEAR SIR,

Hartlepool, near Stockton,
Aug. 19, 1783.

NO perſon could have a better opportunity of diſcerning this awful meteor than myſelf. The weather being, for this climate, aſtoniſhingly hot, my FAHRENHEIT's thermometer, on a north poſition, and in the open air, having for ſeveral days preceding graduated between the hours of ten o'clock in the morning and ſeven o'clock in the evening from 74° to 82°, I ſet out upon a journey to the ſea-ſide. The weather was ſultry, the atmoſphere hazy, and not a breath of air ſtirring. Towards nine o'clock at night it was ſo dark, that I could ſcarcely diſcern the hedges, road, or even the horſes heads. As we proceeded, I obſerved to my attendants, that there was ſomething ſingularly ſtriking in the appearance of the night, not merely from its ſtillneſs and darkneſs, but from the ſulphureous vapours which ſeemed to ſurround us on every ſide. In the midſt of this gloom, and on an inſtant, a brilliant tremulous light appeared to the N. W. by N. At the firſt it ſeemed ſtationary; but in a ſmall ſpace of time it burſt from its poſition, and took its courſe to the S. E. by E. It paſſed directly over our heads with a buzzing noiſe, ſeemingly at the height of ſixty yards. Its tail, as far as the eye could form any judgement, was about eight or ten yards in length. At laſt, this wonderful meteor divided into ſeveral glowing parts or balls of fire, the chief part ſtill remaining in its full ſplendor. Soon after this I heard two great exploſions, each equal to the report of a canon carrying a nine-pound ball. During its awful progreſs, the whole of the atmoſphere, as far as I could diſcern, was perfectly illuminated with the moſt beautifully vivid light I ever remember to have ſeen. The horſes on which we rode ſhrunk with fear; and ſome people whom we met upon the road declared their conſternation in the moſt expreſſive terms.

I have the honor to be, *&c.*

WILLIAM COOPER.

XII. *An Account of the Meteor of the 18th of* Auguſt, 1783. *In a Letter from* Richard Lovell Edgeworth, *Eſq. F.R.S. to Sir* Joſeph Banks, *Bart. P.R.S.*

Read Jan. 15, 1784.

Edgeworthſtown, Mullingar, Ireland.

DEAR SIR,

AT half paſt nine in the evening of the 18th of Auguſt, I ſaw the meteor which has been obſerved in ſo many different places.

Its ſize appeared to be about one third of the moon's diameter; and it moved from the north with an equable velocity, at an elevation of ten or twelve degrees, and in a line parallel to the horizon.

It was viſible during ten or fifteen ſeconds, and ſeemed to be of a parabolic figure, with a luminous tail, twenty or five and twenty of its diameters in length.

It exhibited the moſt vivid colours; the foremoſt part being of the brighteſt blue, followed by different ſhades of red. Twice during its flight it was eclipſed or extinguiſhed, not gradually, but at once, immerging and emerging with undiminiſhed luſtre.

I ſhall not venture to trouble you with any conjectures upon the nature of this phænomenon, as it is probable, that the ſubject has been fully diſcuſſed long before this time by your friends in London. I am, *&c.*

Sept. 5, 1783. RICHARD LOVELL EDGEWORTH.

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XIII. *Experiments on Air. By* Henry Cavendiſh, *Eſq. F.R.S. & S.A.*

Read Jan. 15, 1784.

THE following experiments were made principally with a view to find out the cauſe of the diminution which common air is well known to ſuffer by all the various ways in which it is phlogiſticated, and to diſcover what becomes of the air thus loſt or condenſed; and as they ſeem not only to determine this point, but alſo to throw great light on the conſtitution and manner of production of dephlogiſticated air, I hope they may be not unworthy the acceptance of this ſociety.

Many gentlemen have ſuppoſed that fixed air is either generated or ſeparated from atmoſpheric air by phlogiſtication, and that the obſerved diminution is owing to this cauſe; my firſt experiments therefore were made in order to aſcertain whether any fixed air is really produced thereby. Now, it muſt be obſerved, that as all animal and vegetable ſubſtances contain fixed air, and yield it by burning, diſtillation, or putrefaction, nothing can be concluded from experiments in which the air is phlogiſticated by them. The only methods I know, which are not liable to objection, are by the calcination of metals, the burning of ſulphur or phoſphorus, the mixture of nitrous air, and the exploſion of inflammable air. Perhaps it may be ſuppoſed, that I ought to add to theſe the electric ſpark; but I think it much moſt likely, that the phlogiſtication of the air, and production of fixed air, in this proceſs, is owing to the burning of ſome inflammable matter in the apparatus. When the ſpark is taken from a ſolution of tournſol, the burning of the tournſol may produce this effect; when it is taken from lime-water, the burning of ſome foulneſs adhering to the tube, or perhaps of ſome inflammable matter contained in the lime, may have the ſame effect; and when quickſilver or metallic knobs are uſed, the calcination of them may contribute to the phlogiſtication of the air, though not to the production of fixed air.

There is no reaſon to think that any fixed air is produced by the firſt method of phlogiſtication. Dr. Priestley never found lime-water to become turbid by the calcination of metals over it*: Mr. Lavoisier alſo found only a very ſlight and ſcarce perceptible turbid appearance, without any precipitation, to take place when lime-water was ſhaken in a glaſs veſſel full of the air in which lead had been calcined; and even this ſmall diminution of tranſparency in the lime-water might very likely ariſe, not from fixed air, but only from its being fouled by particles of the calcined metal, which we are told adhered in ſome places to the glaſs. This want of turbidity has been attributed to the fixed air uniting to the metallic calx, in preference to the lime; but there is no reaſon for ſuppoſing that the calx contained any fixed air; for I do not know that any one has extracted it from calces prepared in this manner; and though moſt metallic calces prepared over the fire, or by long expoſure to the atmoſphere, where they are in contact with fixed air, contain that ſubſtance, it by no means follows that they muſt do ſo when prepared by methods in which they are not in contact with it.

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* Experiments on Air, vol. I. p. 137.

Dr. PRIESTLEY alſo obſerved, that quickſilver, fouled by the addition of lead or tin, depoſits a powder by agitation and expoſure to the air, which conſiſts in great meaſure of the calx of the imperfect metal. He found too ſome powder of this kind to contain fixed air *; but it is by no means clear that this air was produced by the phlogiſtication of the air in which the quickſilver was ſhaken; as the powder was not prepared on purpoſe, but was procured from quickſilver fouled by having been uſed in various experiments, and may therefore have contained other impurities beſides the metallic calces.

I never heard of any fixed air being produced by the burning of ſulphur or phoſphorus; but it has been aſſerted, and commonly believed, that lime water is rendered cloudy by a mixture of common and nitrous air; which, if true, would be a convincing proof that on mixing thoſe two ſubſtances ſome fixed air is either generated or ſeparated; I therefore examined this carefully. Now it muſt be obſerved, that as common air uſually contains a little fixed air, which is no eſſential part of it, but is eaſily ſeparated by lime-water; and as nitrous air may alſo contain fixed air, either if the metal from which it is procured be ruſty, or if the water of the veſſel in which it is caught contain calcareous earth, ſuſpended by fixed air, as moſt waters do, it is proper firſt to free both airs from it by previouſly waſhing them with lime water †. Now I found, by repeated

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* Exper. in Nat. Phil. vol. I. p. 144.

† Though fixed air is abſorbed in conſiderable quantity by water, as I ſhewed in Phil. Tranſ. vol. LVI. yet it is not eaſy to deprive common air of all the fixed air

ed experiments, that if the lime water was clean, and the two airs were previouſly waſhed with that ſubſtance, not the leaſt cloud was produced, either immediately on mixing them, or on ſuffering them to ſtand upwards of an hour, though it appeared by the thick clouds which were produced in the lime water, by breathing through it after the experiment was finiſhed, that it was more than ſufficient to ſaturate the acid formed by the decompoſition of the nitrous air, and conſequently that if any fixed air had been produced, it muſt have become viſible. Once indeed I found a ſmall cloud to be formed on the ſurface, after the mixture had ſtood a few minutes. In this experiment the lime water was not quite clean; but whether the cloud was owing to this circumſtance, or to the air's having not been properly waſhed, I cannot pretend to ſay.

Neither does any fixed air ſeem to be produced by the exploſion of the inflammable air obtained from metals, with either common or dephlogiſticated air. This I tried by putting a little lime-water into a glaſs globe fitted with a braſs cock, ſo as to make it air tight, and an apparatus for firing air by electricity. This globe was exhauſted by an air-pump, and the two airs, which had been previouſly waſhed with lime-water, let in, and ſuffered to remain ſome time, to ſhew whether they would affect the lime water, and then fired by electricity. The event was, that not the leaſt cloud was produced in the lime-water, when the inflammable air was mixed with common air, and

air contained in it by means of water. On ſhaking a mixture of ten parts of common air, and one of fixed air, with more than an equal bulk of diſtilled water, not more than half of the fixed air was abſorbed, and on transferring the air into freſh diſtilled water only half the remainder was abſorbed, as appeared by the diminution which it ſtill ſuffered on adding lime water.

only a very ſlight one, or rather diminution of tranſparency, when it was combined with dephlogiſticated air. This, however, ſeemed not to be produced by fixed air; as it appeared inſtantly after the exploſion, and did not increaſe on ſtanding, and was ſpread uniformly through the liquor; whereas if it had been owing to fixed air, it would have taken up ſome ſhort time before it appeared, and would have begun firſt at the ſurface, as was the caſe in the abovementioned experiment with nitrous air. What it was really owing to I cannot pretend to ſay; but if it did proceed from fixed air it would ſhew that only an exceſſively minute quantity was produced *. On the whole, though it is not improbable that fixed air may be generated in ſome chymical proceſſes, yet it ſeems certain that it is not the general effect of phlogiſticating air, and that the diminution of common air is by no means owing to the generation or ſeparation of fixed air from it.

As there ſeemed great reaſon to think, from Dr. Prieſtley's experiments, that the nitrous and vitriolic acids were convertible into dephlogiſticated air, I tried whether the dephlogiſticated part of common air might not, by phlogiſtication, be changed into nitrous or vitriolic acid. For this purpoſe I impregnated ſome milk of lime with the fumes of burning ſulphur, by putting a little of it into a large glaſs receiver, and burning ſulphur therein, taking care to keep the mouth of the receiver ſtopt till the fumes were all abſorbed; after which the air of the receiver was changed, and more ſulphur burnt in it as before, and the proceſs repeated till 122 grains of ſulphur were conſumed. The milk of lime was then filtered and evaporated, but it yielded no nitrous ſalt, nor any other ſubſtance except ſelenite; ſo that no ſenſible quantity of the air was changed

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* Dr. PRIESTLEY alſo found no fixed air to be produced by the exploſion of inflammable and common air. Vol. V. p. 124.

into nitrous acid. It muſt be obſerved, that as the vitriolic acid produced by the burning ſulphur is changed by its union with the lime into ſelenite, which is very little ſoluble in water, a very ſmall quantity of nitrous ſalt, or any other ſubſtance which is ſoluble in water, would have been perceived.

I alſo tried whether any nitrous acid was produced by phlogiſticating common air with liver of ſulphur; for this purpoſe I made a ſolution of flowers of ſulphur by boiling it with lime, and put a little of it into a large receiver, and ſhook it frequently, changing now and then the air, till the yellow colour of the ſolution was quite gone; a ſign that all the ſulphur was, by the loſs of its phlogiſton, turned into vitriolic acid, and united to the lime, or precipitated; the liquor was then filtered and evaporated, but it yielded not the leaſt nitrous ſalt.

The experiment was repeated in nearly the ſame manner with dephlogiſticated air procured from red precipitate; but not the leaſt nitrous acid was obtained.

It is well known that common ſelenite is very little ſoluble in water; whereas that procured in the two laſt experiments was very ſoluble, and even cryſtallized readily, and was intenſely bitter; this however appeared to be owing merely to the acid with which it was formed being very much phlogiſticated; for on evaporating it to dryneſs, and expoſing it to the air for a few days, it became much leſs ſoluble, ſo that on adding water to it not much diſſolved; and by repeating this proceſs once or twice, it ſeemed to become not more ſoluble than ſelenite made in the common manner.

This ſolubility of the ſelenite cauſed ſome trouble in trying the experiment; for while it continued much ſoluble it would have been impoſſible to have diſtinguiſhed a ſmall mixture of nitrous ſalt; but by the abovementioned proceſs I was able to diſtinguiſh as ſmall a proportion as if the ſelenite had been originally no more ſoluble than uſual.

The nature of the neutral ſalts made with the phlogiſticated vitriolic and nitrous acids has not been much examined by the chymiſts, though it ſeems well worth their attention; and it is likely that many beſides the foregoing may differ remarkably from thoſe made with the ſame acids in their common ſtate. Nitre formed with the phlogiſticated nitrous acid has been found to differ conſiderably from common nitre, as well as Sal. Polychreſt from vitriolated tartar.

In order to try whether any vitriolic acid was produced by the phlogiſtication of air, I impregnated fifty ounces of diſtilled water with the fumes produced on mixing fifty-two ounce meaſures of nitrous air with a quantity of common air ſufficient to decompound it. This was done by filling a bottle with ſome of this water, and inverting it into a baſon of the ſame, and then, by a ſyphon, letting in as much nitrous air as filled it half-full; after which common air was added ſlowly by the ſame ſyphon, till all the nitrous air was decompounded. When this was done, the diſtilled water was further impregnated in the ſame manner till the whole of the abovementioned quantity of nitrous air was employed. This impregnated water, which was very ſenſibly acid to the taſte, was diſtilled in a glaſs retort. The firſt runnings were very acid, and ſmelt pungent, being nitrous acid much phlogiſticated; what came next had no ſenſible taſte or ſmell; but the laſt runnings were very acid, and conſiſted of nitrous acid not phlogiſticated. Scarce any ſediment was left behind. Theſe different parcels of diſtilled liquor were then exactly ſaturated with ſalt of tartar, and evaporated; they yielded 87½ grains of nitre, which, as far as I could perceive, was unmixed with vitriolated tartar or any

other

other ſubſtance, and conſequently no ſenſible quantity of the common air with which the nitrous air was mixed was turned into vitriolic acid.

It appears, from this experiment, that nitrous air contains as much acid as 2¾ times its weight of ſaltpetre; for fifty-two ounce meaſures of nitrous air weigh 32 grains, and, as was before ſaid, yield as much acid as is contained in 87½ grains of ſaltpetre; ſo that the acid in nitrous air is in a remarkably concentrated ſtate, and I believe more than 1½ times as much ſo as the ſtrongeſt ſpirit of nitre ever prepared.

Having now mentioned the unſucceſsful attempts I made to find out what becomes of the air loſt by phlogiſtication, I proceed to ſome experiments, which ſerve really to explain the matter.

In Dr. Priestley's laſt volume of experiments is related an experiment of Mr. Warltire's, in which it is ſaid that, on firing a mixture of common and inflammable air by electricity in a cloſe copper veſſel holding about three pints, a loſs of weight was always perceived, on an average about two grains, though the veſſel was ſtopped in ſuch a manner that no air could eſcape by the exploſion. It is alſo related, that on repeating the experiment in glaſs veſſels, the inſide of the glaſs, though clean and dry before, immediately became dewy; which confirmed an opinion he had long entertained, that common air depoſits its moiſture by phlogiſtication. As the latter experiment ſeemed likely to throw great light on the ſubject I had in view, I thought it well worth examining more cloſely. The firſt experiment alſo, if there was no miſtake in it, would be very extraordinary and curious; but it did not ſucceed with me; for though the veſſel I uſed held more than Mr. Warltire's, namely, 24,000 grains of water, and though the experiment was repeated ſeveral times with different proportions of common and inflammable air, I could never perceive a loſs of weight of more than one-fifth of a grain, and commonly none at all. It muſt be obſerved, however, that though there were ſome of the experiments in which it ſeemed to diminiſh a little in weight, there were none in which it increaſed*.

In all the experiments, the inſide of the glaſs globe became dewy, as obſerved by Mr. WARLTIRE; but not the leaſt ſooty matter could be perceived. Care was taken in all of them to find how much the air was diminiſhed by the exploſion, and to obſerve its teſt. The reſult is as follows: the bulk of the inflammable air being expreſſed in decimals of the common air,

Common air.	Inflammable air.	Diminution.	Air remaining after the exploſion.	Teſt of this air in firſt method.	Standard.
1	1,241	,686	1,555	,055	,0
	1,055	,642	1,413	,063	,0
	,706	,647	1,059	,066	,0
	,423	,612	,811	,097	,03
	,331	,476	,855	,339	,27
	,206	,294	,912	,048	,58

In theſe experiments the inflammable air was procured from zinc, as it was in all my experiments, except where otherwiſe expreſſed: but I made two more experiments, to try whether there was any difference between the air from zinc and that from iron, the quantity of inflammable air being the ſame in both, namely, 0,331 of the common; but I could not find any difference to be depended on between the two kinds of air, either in the diminution which they ſuffered by the exploſion, or the teſt of the burnt air.

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* Dr. PRIESTLEY, I am informed, has ſince found the experiment not to ſucceed.

From the fourth experiment it appears, that 423 meaſures of inflammable air are nearly ſufficient to completely phlogiſticate 1000 of common air; and that the bulk of the air remaining after the exploſion is then very little more than four-fifths of the common air employed; ſo that as common air cannot be reduced to a much leſs bulk than that by any method of phlogiſtication, we may ſafely conclude, that when they are mixed in this proportion, and exploded, almoſt all the inflammable air, and about one-fifth part of the common air, loſe their elaſticity, and are condenſed into the dew which lines the glaſs.

The better to examine the nature of this dew, 500000 grain meaſures of inflammable air were burnt with about 2½ times that quantity of common air, and the burnt air made to paſs through a glaſs cylinder eight feet long and three-quarters of an inch in diameter, in order to depoſit the dew. The two airs were conveyed ſlowly into this cylinder by ſeparate copper pipes, paſſing through a braſs plate which ſtopped up the end of the cylinder; and as neither inflammable nor common air can burn by themſelves, there was no danger of the flame ſpreading into the magazines from which they were conveyed. Each of theſe magazines conſiſted of a large tin veſſel, inverted into another veſſel juſt big enough to receive it. The inner veſſel communicated with the copper pipe, and the air was forced out of it by pouring water into the outer veſſel; and in order that the quantity of common air expelled ſhould be 2½ times that of the inflammable, the water was let into the outer veſſels by two holes in the bottom of the ſame tin pan, the hole which conveyed the water into that veſſel in which the common air was confined being 2½ times as big as the other.

In trying the experiment, the magazines being firſt filled with their reſpective airs, the glaſs cylinder was taken off, and water let, by the two holes, into the outer veſſels, till the airs began to iſſue from the ends of the copper pipes; they were then ſet on fire by a candle, and the cylinder put on again in its place. By this means upwards of 135 grains of water were condenſed in the cylinder, which had no taſte nor ſmell, and which left no ſenſible ſediment when evaporated to dryneſs; neither did it yield any pungent ſmell during the evaporation; in ſhort, it ſeemed pure water.

In my firſt experiment, the cylinder near that part where the air was fired was a little tinged with ſooty matter, but very ſlightly ſo; and that little ſeemed to proceed from the putty with which the apparatus was luted, and which was heated by the flame; for in another experiment, in which it was contrived ſo that the luting ſhould not be much heated, ſcarce any ſooty tinge could be perceived.

By the experiments with the globe it appeared, that when inflammable and common air are exploded in a proper proportion, almoſt all the inflammable air, and near one-fifth of the common air, loſe their elaſticity, and are condenſed into dew. And by this experiment it appears, that this dew is plain water, and conſequently that almoſt all the inflammable air, and about one-fifth of the common air, are turned into pure water.

In order to examine the nature of the matter condenſed on firing a mixture of dephlogiſticated and inflammable air, I took a glaſs globe, holding 8800 grain measures, furniſhed with a braſs cock and an apparatus for firing air by electricity. This globe was well exhauſted by an air-pump, and then filled with a mixture of inflammable and dephlogiſticated air, by ſhutting the cock, faſtening a bent glaſs tube to its mouth, and letting up the end of it into a glaſs jar inverted into water, and containing a mixture of 19500 grain meaſures of dephlogiſticated air, and 37000 of inflammable; ſo that, upon opening the cock, ſome of this mixed air ruſhed through the bent tube, and filled the globe *. The cock was then ſhut, and the included air fired by electricity, by which means almoſt all of it loſt its elaſticity. The cock was then again opened, ſo as to let in more of the ſame air, to ſupply the place of that deſtroyed by the exploſion, which was again fired, and the operation continued till almoſt the whole of the mixture was let into the globe and exploded. By this means, though the globe held not more than the ſixth part of the mixture, almoſt the whole of it was exploded therein, without any freſh exhauſtion of the globe.

As I was deſirous to try the quantity and teſt of this burnt air, without letting any water into the globe, which would have prevented my examining the nature of the condenſed matter, I took a larger globe, furniſhed alſo with a ſtop cock, exhauſted it by an air-pump, and ſcrewed it on upon the cock of the former globe; upon which, by opening both cocks, the air ruſhed out of the ſmaller globe into the larger, till it became of equal denſity in both; then, by ſhutting the cock of the larger globe, unſcrewing it again from the former, and opening it under water, I was enabled to find the quantity of the burnt air in it; and conſequently, as the proportion which the contents of the two globes bore to each other was

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* In order to prevent any water from getting into this tube, while dipped under water to let it up into the glaſs jar, a bit of wax was was ſtuck upon the end of it, which was rubbed off when raiſed above the ſurface of the water.

known, could tell the quantity of burnt air in the ſmall globe before the communication was made between them. By this means the whole quantity of the burnt air was found to be 2950 grain meaſures; its ſtandard was 1,85.

The liquor condenſed in the globe, in weight about 30 grains, was ſenſibly acid to the taſte, and by ſaturation with fixed alkali, and evaporation, yielded near two grains of nitre; ſo that it conſiſted of water united to a ſmall quantity of nitrous acid. No ſooty matter was depoſited in the globe. The dephlogiſticated air uſed in this experiment was procured from red precipitate, that is, from a ſolution of quickſilver in ſpirit of nitre diſtilled till it acquires a red colour.

As it was ſuſpected, that the acid contained in the condenſed liquor was no eſſential part of the dephlogiſticated air, but was owing to ſome acid vapour which came over in making it and had not been abſorbed by the water, the experiment was repeated in the ſame manner, with ſome more of the ſame air, which had been previouſly waſhed with water, by keeping it a day or two in a bottle with ſome water, and ſhaking it frequently; whereas that uſed in the preceding experiment had never paſſed through water, except in preparing it. The condenſed liquor was ſtill acid.

The experiment was alſo repeated with dephlogiſticated air, procured from red lead by means of oil of vitriol; the liquor condenſed was acid, but by an accident I was prevented from determining the nature of the acid.

I alſo procured ſome dephlogiſticated air from the leaves of plants, in the manner of Doctors INGENHOUSZ and PRIESTLEY, and exploded it with inflammable air as before; the condenſed liquor ſtill continued acid, and of the nitrous kind.

In all theſe experiments the proportion of inflammable air was ſuch, that the burnt air was not much phlogiſticated; and it was obſerved, that the leſs phlogiſticated it was, the more acid was the condenſed liquor. I therefore made another experiment, with ſome more of the ſame air from plants, in which the proportion of inflammable air was greater, ſo that the burnt air was almoſt completely phlogiſticated, its ſtandard being $\frac{4}{10}$. The condenſed liquor was then not at all acid, but ſeemed pure water: ſo that it appears, that with this kind of dephlogiſticated air, the condenſed liquor is not at all acid, when the two airs are mixed in ſuch a proportion that the burnt air is almoſt completely phlogiſticated, but is conſiderably ſo when it is not much phlogiſticated.

In order to ſee whether the ſame thing would obtain with air procured from red precipitate, I made two more experiments with that kind of air, the air in both being taken from the ſame bottle, and the experiment tried in the ſame manner, except that the proportions of inflammable air were different. In the firſt, in which the burnt air was almoſt completely phlogiſticated, the condenſed liquor was not at all acid. In the ſecond, in which its ſtandard was 1,86, that is, not much phlogiſticated, it was conſiderably acid; ſo that with this air, as well as with that from plants, the condenſed liquor contains, or is entirely free from, acid, according as the burnt air is leſs or more phlogiſticated; and there can be little doubt but that the ſame rule obtains with any other kind of dephlogiſticated air.

In order to ſee whether the acid, formed by the exploſion of dephlogiſticated air obtained by means of the vitriolic acid, would alſo be of the nitrous kind, I procured ſome air from turbith mineral, and exploded it with inflammable air, the proportion being ſuch that the burnt air was not much phlogiſticated. The condenſed liquor manifeſted an acidity, which appeared, by ſaturation with a ſolution of ſalt of tartar, to be of the nitrous kind; and it was found, by the addition of ſome terra ponderoſa ſalita, to contain little or no vitriolic acid.

When inflammable air was exploded with common air, in ſuch a proportion that the ſtandard of the burnt air was about $\frac{4}{10}$, the condenſed liquor was not in the leaſt acid. There is no difference, however, in this reſpect between common air, and dephlogiſticated air mixed with phlogiſticated in ſuch a proportion as to reduce it to the ſtandard of common air; for ſome dephlogiſticated air from red precipitate, being reduced to this ſtandard by the addition of perfectly phlogiſticated air, and then exploded with the ſame proportion of inflammable air as the common air was in the foregoing experiment, the condenſed liquor was not in the leaſt acid.

From the foregoing experiments it appears, that when a mixture of inflammable and dephlogiſticated air is exploded in ſuch proportion that the burnt air is not much phlogiſticated, the condenſed liquor contains a little acid, which is always of the nitrous kind, whatever ſubſtance the dephlogiſticated air is procured from; but if the proportion be ſuch that the burnt air is almoſt entirely phlogiſticated, the condenſed liquor is not at all acid, but ſeems pure water, without any addition whatever; and as, when they are mixed in that proportion, very little air remains after the exploſion, almoſt the whole being condenſed, it follows, that almoſt the whole of the inflammable and dephlogiſticated air is converted into pure water. It is not eaſy, indeed, to determine from theſe experiments what proportion the burnt air, remaining after the exploſions, bore to the dephlogiſticated air employed, as neither the ſmall nor the large globe could be perfectly exhauſted of air, and there was no ſaying with exactneſs what quantity was left in them; but in moſt of them, after allowing for this uncertainty, the true quantity of burnt air ſeemed not more than $\frac{1}{50}$th of the dephlogiſticated air employed, or $\frac{1}{150}$th of the mixture. It ſeems, however, unneceſſary to determine this point exactly, as the quantity is ſo ſmall, that there can be little doubt but that it proceeds only from the impurities mixed with the dephlogiſticated and inflammable air, and conſequently that, if thoſe airs could be obtained perfectly pure, the whole would be condenſed.

With reſpect to common air, and dephlogiſticated air reduced by the addition of phlogiſticated air to the ſtandard of common air, the caſe is different; as the liquor condenſed in exploding them with inflammable air, I believe I may ſay in any proportion, is not at all acid; perhaps, becauſe if they are mixed in ſuch a proportion as that the burnt air is not much phlogiſticated, the exploſion is too weak, and not accompanied with ſufficient heat.

All the foregoing experiments, on the exploſion of inflammable air with common and dephlogiſticated airs, except thoſe which relate to the cauſe of the acid found in the water, were made in the ſummer of the year 1781, and were mentioned by me to Dr. PRIESTLEY, who in conſequence of it made ſome experiments of the ſame kind, as he relates in a paper printed in the preceding volume of the Tranſactions. During the laſt ſummer alſo, a friend of mine gave ſome account of them to M. LAVOISIER, as well as of the concluſion drawn from them, that dephlogiſticated air is only water deprived of phlogiſton; but at that time ſo far was M. LAVOISIER from thinking any ſuch opinion warranted, that, till he was prevailed upon to repeat the experiment himſelf, he found ſome difficulty in believing that nearly the whole of the two airs could be converted into water. It is remarkable, that neither of theſe gentlemen found any acid in the water produced by the combuſtion; which might proceed from the latter having burnt the two airs in a different manner from what I did; and from the former having uſed a different kind of inflammable air, namely, that from charcoal, and perhaps having uſed a greater proportion of it.

Before I enter into the cauſe of theſe phænomena, it will be proper to take notice, that phlogiſticated air appears to be nothing elſe than the nitrous acid united to phlogiſton; for when nitre is deflagrated with charcoal, the acid is almoſt entirely converted into this kind of air. That the acid is entirely converted into air, appears from the common proceſs for making what is called clyſſus of nitre; for if the nitre and charcoal are dry, ſcarce any thing is found in the veſſels prepared for condenſing the fumes; but if they are moiſt a little liquor is collected, which is nothing but the water contained in the materials, impregnated with a little volatile alkali, proceeding in all probability from the imperfectly burnt charcoal, and a little fixed alkali, conſiſting of ſome of the alkalized nitre carried over by the heat and watery vapours. As far as I can perceive too, at preſent, the air into which much the greateſt part of the acid is converted, differs in no reſpect from common air phlogiſticated. A ſmall part of the acid, however, is turned into nitrous air, and the whole is mixed with a good deal of fixed, and perhaps a little inflammable air, both proceeding from the charcoal.

It is well known, that the nitrous acid is alſo converted by phlogiſtication into nitrous air, in which reſpect there ſeems a conſiderable analogy between that and the vitriolic acid; for the vitriolic acid, when united to a ſmaller proportion of phlogiſton, forms the volatile ſulphureous acid and vitriolic acid air, both of which, by expoſure to the atmoſphere, loſe their phlogiſton, though not very faſt, and are turned back into vitriolic acid; but, when united to a greater proportion of phlogiſton, it forms ſulphur, which ſhews no ſigns of acidity, unleſs a ſmall degree of affinity to alkalies can be called ſo, and in which the phlogiſton is more ſtrongly adherent, ſo that it does not fly off when expoſed to the air, unleſs aſſiſted by a heat ſufficient to ſet it on fire. In like manner the nitrous acid, united to a certain quantity of phlogiſton, forms nitrous fumes and nitrous air, which readily quit their phlogiſton to common air; but when united to a different, in all probability a larger quantity, it forms phlogiſticated air, which ſhews no ſigns of acidity, and is ſtill leſs diſpoſed to part with its phlogiſton than ſulphur.

This being premiſed, there ſeem two ways by which the phænomena of the acid found in the condenſed liquor may be explained; firſt, by ſuppoſing that dephlogiſticated air contains a little nitrous acid which enters into it as one of its component parts, and that this acid, when the inflammable air is in a ſufficient proportion, unites to the phlogiſton, and is turned into phlogiſticated air, but does not when the inflammable air is in too ſmall a proportion; and, ſecondly, by ſuppoſing that there is no nitrous acid mixed with, or entering into the compoſition of, dephlogiſticated air, but that, when this air is in a ſufficient proportion, part of the phlogiſticated air with which it is debaſed is, by the ſtrong affinity of phlogiſton to dephlogiſticated air, deprived of its phlogiſton and turned into nitrous acid; whereas, when the dephlogiſticated air is not more than ſufficient to conſume the inflammable air, none then remains to deprive the phlogiſticated air of its phlogiſton, and turn it into acid.

If the latter explanation be true, I think, we muſt allow that dephlogiſticated air is in reality nothing but dephlogiſticated water, or water deprived of its phlogiſton; or, in other words, that water conſiſts of dephlogiſticated air united to phlogiſton; and that inflammable air is either pure phlogiſton, as Dr. PRIESTLEY and Mr. KIRWAN ſuppoſe, or elſe water united to phlogiſton*; ſince, according to this ſuppoſition, theſe two ſubſtances united together form pure water. On the other hand, if the firſt explanation be true, we muſt ſuppoſe that dephlogiſticated air conſiſts of water united to a little nitrous acid and deprived of its phlogiſton; but ſtill the nitrous acid in it muſt make only a very ſmall part of the whole, as it is found, that the phlogiſticated air, which it is converted into, is very ſmall in compariſon of the dephlogiſticated air.

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* Either of theſe ſuppoſitions will agree equally well with the following experiments; but the latter ſeems to me much the moſt likely. What principally makes me think ſo is, that common or dephlogiſticated air do not abſorb phlogiſton from inflammable air, unleſs aſſiſted by a red heat, whereas they abſorb the phlogiſton of nitrous air, liver of ſulphur, and many other ſubſtances, without that aſſiſtance; and it ſeems inexplicable, that they ſhould refuſe to unite to pure phlogiſton, when they are able to extract it from ſubſtances to which it has an affinity; that is, that they ſhould overcome the affinity of phlogiſton to other ſubſtances, and extract it from them, when they will not even unite to it when preſented to them. On the other hand, I know no experiment which ſhews inflammable air to be pure phlogiſton rather than an union of it with water, unleſs it be Dr. PRIESTLEY's experiment of expelling inflammable air from iron by heat alone. I am not ſufficiently acquainted with the circumſtances of that experiment to argue with certainty about it; but I think it much more likely, that the inflammable air was formed by the union of the phlogiſton of the iron filings with the water diſperſed among them, or contained in the retort or other veſſel in which it was heated; and in all probability this was the cauſe of the ſeparation of the phlogiſton, as iron ſeems not diſpoſed to part with its phlogiſton by heat alone, without being aſſiſted by the air or ſome other ſubſtance.

I think the ſecond of theſe explanations ſeems much the moſt likely; as it was found, that the acid in the condenſed liquor was of the nitrous kind, not only when the dephlogiſticated air was prepared from red precipitate, but alſo when it was procured from plants or from turbith mineral: and it ſeeems not likely, that air procured from plants, and ſtill leſs likely that air procured from a ſolution of mercury in oil of vitriol, ſhould contain any nitrous acid.

Another ſtrong argument in favour of this opinion is, that dephlogiſticated air yields no nitrous acid when phlogiſticated by liver of ſulphur; for if this air contains nitrous acid, and yields it when phlogiſticated by exploſion with inflammable air, it is very extraordinary that it ſhould not do ſo when phlogiſticated by other means.

But what forms a ſtronger and, I think, almoſt deciſive argument in favour of this explanation is, that when the dephlogiſticated air is very pure, the condenſed liquor is made much more ſtrongly acid by mixing the air to be exploded with a little phlogiſticated air, as appears by the following experiments.

A mixture of 18500 grain meaſures of inflammable air with 9750 of dephlogiſticated air procured from red precipitate were exploded in the uſual manner; after which, a mixture of the ſame quantities of the ſame dephlogiſticated and inflammable air, with the addition of 2500 of air phlogiſticated by iron filings and ſulphur, was treated in the ſame manner. The condenſed liquor, in both experiments, was acid, but that in the latter evidently more ſo, as appeared alſo by ſaturating each of them ſeparately with marble powder, and precipitating

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the earth by fixed alkali, the precipitate of the ſecond experiment weighing one-fifth of a grain, and that of the firſt being ſeveral times leſs. The ſtandard of the burnt air in the firſt experiment was 1,86, and in the ſecond only 0,9.

It muſt be obſerved, that all circumſtances were the ſame in theſe two experiments, except that in the latter the air to be exploded was mixed with ſome phlogiſticated air, and that in conſequence the burnt air was more phlogiſticated than in the former; and from what has been before ſaid, it appears, that this latter circumſtance ought rather to have made the condenſed liquor leſs acid; and yet it was found to be much more ſo, which ſhews ſtrongly that it was the phlogiſticated air which furniſhed the acid.

As a further confirmation of this point, theſe two comparative experiments were repeated with a little variation, namely, in the firſt experiment there was firſt let into the globe 1500 of dephlogiſticated air, and then the mixture, conſiſting of 12200 of dephlogiſticated air and 25900 of inflammable, was let in at different times as uſual. In the ſecond experiment, beſides the 1500 of dephlogiſticated air firſt let in, there was alſo admitted 2500 of phlogiſticated air, after which the mixture, conſiſting of the ſame quantities of dephlogiſticated and inflammable air as before, was let in as uſual. The condenſed liquor of the ſecond experiment was about three times as acid as that of the firſt, as it required 119 grains of a diluted ſolution of ſalt of tartar to ſaturate it, and the other only 37. The ſtandard of the burnt air was 0,78 in the ſecond experiment, and 1,96 in the firſt.

The intention of previouſly letting in ſome dephlogiſticated air in the two laſt experiments was, that the condenſed liquor was expected to become more acid thereby, as proved actually to be the caſe.

In the firſt of theſe two experiments, in order that the air to be exploded ſhould be as free as poſſible from common air, the globe was firſt filled with a mixture of dephlogiſticated and inflammable air, it was then exhauſted, and the air to be exploded let in; by which means, though the globe was not perfectly exhauſted, very little common air could be left in it. In the firſt ſet of experiments this circumſtance was not attended to, and the purity of the dephlogiſticated air was forgot to be examined in both ſets.

From what has been ſaid there ſeems the utmoſt reaſon to think, that dephlogiſticated air is only water deprived of its phlogiſton, and that inflammable air, as was before ſaid, is either phlogiſticated water, or elſe pure phlogiſton; but in all probability the former.

As Mr. WATT, in a paper lately read before this Society, ſuppoſes water to conſiſt of dephlogiſticated air and phlogiſton deprived of part of their latent heat, whereas I take no notice of the latter circumſtance, it may be proper to mention in a few words the reaſon of this apparent difference betwen us. If there be any ſuch thing as elementary heat, it muſt be allowed that what Mr. WATT ſays is true; but by the ſame rule we ought to ſay, that the diluted mineral acids conſiſt of the concentrated acids united to water and deprived of part of their latent heat; that ſolutions of ſal ammoniac, and moſt other other neutral ſalts, conſiſt of the ſalt united to water and elementary heat; and a ſimilar language ought to be uſed in ſpeaking of almoſt all chemical combinations, as there are very few which are not attended with ſome increaſe or diminution of heat. Now I have choſen to avoid this form of ſpeaking, both becauſe I think it more likely that there is no ſuch thing as elementary heat, and becauſe ſaying ſo in this inſtance, without uſing ſimilar expreſſions in ſpeaking of other chemical unions, would be improper, and would lead to falſe ideas; and it may even admit of doubt, whether the doing it in general would not cauſe more trouble and perplexity than it is worth.

There is the utmoſt reaſon to think, that dephlogiſticated and phlogiſticated air, as M. LAVOISIER and SCHEELE ſuppoſe, are quite diſtinct ſubſtances, and not differing only in their degree of phlogiſtication; and that common air is a mixture of the two; for if the dephlogiſticated air is pretty pure, almoſt the whole of it loſes its elaſticity by phlogiſtication, and, as appears by the foregoing experiments, is turned into water, inſtead of being converted into phlogiſticated air. In moſt of the foregoing experiments, at leaſt $\frac{16}{17}$ths of the whole was turned into water; and by treating ſome dephlogiſticated air with liver of ſulphur, I have reduced it to leſs than $\frac{1}{10}$th of its original bulk, and other perſons, I believe, have reduced it to a ſtill leſs bulk; ſo that there ſeems the utmoſt reaſon to ſuppoſe, that the ſmall reſiduum which remains after its phlogiſtication proceeds only from the impurities mixed with it.

It was juſt ſaid, that ſome dephlogiſticated air was reduced by liver of ſulphur to $\frac{1}{10}$th of its original bulk; the ſtandard of this air was 4,8, and conſequently the ſtandard of perfectly pure dephlogiſticated air ſhould be very nearly 5, which is a confirmation of the foregoing opinion; for if the ſtandard of pure dephlogiſticated air is 5, common air muſt, according to this opinion, contain one-fifth of it, and therefore ought to loſe one-fifth of its bulk by phlogiſtication, which is what it is actually found to loſe.

From what has been ſaid, it follows, that inſtead of ſaying air is phlogiſticated or dephlogiſticated by any means, it would be more ſtrictly juſt to ſay, it is deprived of, or receives, an addition of dephlogiſticated air; but as the other expreſſion is convenient, and can ſcarcely be conſidered as improper, I ſhall ſtill frequently make uſe of it in the remainder of this paper.

There ſeemed great reaſon to think, from Dr. Priestley's experiments, that both the nitrous and vitriolic acids were convertible into dephlogiſticated air, as that air is procured in the greateſt quantity from ſubſtances containing thoſe acids, eſpecially the former. The foregoing experiments, however, ſeem to ſhew that no part of the acid is converted into dephlogiſticated air, and that their uſe in preparing it is owing only to the great power which they poſſeſs of depriving bodies of their phlogiſton. A ſtrong confirmation of this is, that red precipitate, which is one of the ſubſtances yielding dephlogiſticated air in the greateſt quantity, and which is prepared by means of the nitrous acid, contains in reality no acid. This I found by grinding 400 grains of it with ſpirits of ſal ammoniac, and keeping them together for ſome days in a bottle, taking care to ſhake them frequently. The red colour of the precipitate was rendered pale, but not entirely deſtroyed; being then waſhed with water and filtered, the clear liquor yielded on evaporation not the leaſt ammoniacal ſalt.

It is natural to think, that if any nitrous acid had been contained in the red precipitate, it would have united to the volatile alkali and have formed ammoniacal nitre, and would have been perceived on evaporation; but in order to determine more certainly whether this would be the caſe, I dried ſome of the ſame ſolution of quickſilver from which the red precipitate was prepared with a leſs heat, ſo that it acquired only an orange colour, and treated the ſame quantity of it with volatile alkali in the ſame manner as before. It immediately cauſed an efferveſcence, changed the colour to grey, and yielded 52 grains of ammoniacal nitre. There is the utmoſt reaſon to think, therefore, that red precipitate contains no nitrous acid; and conſequently that, in procuring dephlogiſticated air from it, no acid is converted into air; and it is reaſonable to conclude, therefore, that no ſuch change is produced in procuring it from any other ſubſtance.

It remains to conſider in what manner theſe acids act in producing dephlogiſticated air. The way in which the nitrous acid acts, in the production of it from red precipitate, ſeems to be as follows. On diſtilling the mixture of quickſilver and ſpirit of nitre, the acid comes over, loaded with phlogiſton, in the form of nitrous vapour, and continues to do ſo till the remaining matter acquires its full red colour, by which time all the nitrous acid is driven over, but ſome of the watery part ſtill remains behind, and adheres ſtrongly to the quickſilver; ſo that the red precipitate may be conſidered, either as quickſilver deprived of part of its phlogiſton, and united to a certain portion of water, or as quickſilver united to dephlogiſticated air*; after which, on further increaſing the heat, the water in it riſes deprived of its phlogiſton, that is, in the form of dephlogiſticated air, and at the ſame time the quickſilver diſtils over in its metallic form. It is juſtly remarked by Dr. PRIESTLEY, that the ſolution of quickſilver does not begin to yield dephlogiſticated air till it acquires its red colour.

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* Unleſs we were much better acquainted than we are with the manner in which different ſubſtances are united together in compound bodies, it would be ridiculous to ſay, that it is the quickſilver in the red precipitate which is deprived of its phlogiſton, and not the water, or that it is the water and not the quickſilver; all that we can ſay is, that red precipitate conſiſts of quickſilver and water, one or both of which are deprived of part of their phlogiſton. In like manner, during the preparation of the red precipitate, it is certain that the acid abſorbs phlogiſton, either from the quickſilver or the water; but we are by no means authoriſed to ſay from which.

Mercurius calcinatus appears to be only quickſilver which has abſorbed dephlogiſticated air from the atmoſphere during its preparation; accordingly, by giving it a ſufficient heat, the dephlogiſticated air is driven off, and the quickſilver acquires its original form. It ſeems therefore that mercurius calcinatus and red precipitate, though prepared in a different manner, are very nearly the ſame thing.

From what has been ſaid it follows, that red precipitate and mercurius calcinatus contain as much phlogiſton as the quickſilver they are prepared from; but yet, as uniting dephlogiſticated air to a metal comes to the ſame thing as depriving it of part of its phlogiſton and adding water to it, the quickſilver may ſtill be conſidered as deprived of its phlogiſton; but the imperfect metals ſeem not only to abſorb dephlogiſticated air during their calcination, but alſo to be really deprived of part of their phlogiſton, as they do not acquire their metallic form by driving off the dephlogiſticated air.

In procuring dephlogiſticated air from nitre, the acid acts in a different manner, as, upon heating the nitre red-hot, the dephlogiſticated air riſes mixed with a little nitrous acid, and at the ſame time the acid remaining in the nitre becomes very much phlogiſticated; which ſhews that the acid abſorbs phlogiſton from the water in the nitre, and becomes phlogiſticated, while the water is thereby turned into dephlogiſticated air. On diſtilling 3155 grains of nitre in an unglazed earthen retort, it yielded 256000 grain meaſures of dephlogiſticated air*, the ſtandard of different parts of which varied from 3 to 3,65, but at a medium was 3,35. The matter remaining in the retort diſſolved readily in water, and taſted alcaline and cauſtic. On adding diluted ſpirit of nitre to the ſolution, ſtrong red fumes were produced; a ſign that the acid in it was very much phlogiſticated, as no fumes whatever would have been produced on adding the ſame acid to a ſolution of common nitre; that part of the ſolution alſo which was ſuperſaturated with acid became blue; a colour which the diluted nitrous acid is known to aſſume when much phlogiſticated. The ſolution, when ſaturated with this acid, loſt its alcaline and cauſtic taſte, but yet taſted very different from true nitre, ſeeming as if it had been mixed with ſea-ſalt, and alſo required much leſs water to diſſolve it; but on expoſing it for ſome days to the air, and adding freſh acid as faſt as by the flying off of the fumes the alcali predominated, it became true nitre, unmixed, as far as I could perceive, with any other ſalt *.

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* This is, about eighty-one grain meaſures from one grain of nitre; and the weight

It has been remarked, that the dephlogiſticated air procured from nitre is leſs pure, than that from red precipitate and many other ſubſtances, which may perhaps proceed from unglazed earthen retorts having been commonly uſed for this purpoſe, and which, conformably to Dr. PRIESTLEY's diſcovery, may poſſibly abſorb ſome common air from without, and emit it along with the dephlogiſticated air; but if it ſhould be found that the dephlogiſticated air procured from nitre in glaſs or glazed earthen veſſels is alſo impure, it would ſeem to ſhew that part

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weight of the dephlogiſticated air, ſuppoſing it 800 times lighter than water, is one tenth of that of the nitre. In all probability it would have yielded a much greater quantity of air, if a greater heat had been applied.

* This phlogiſtication of the acid in nitre by heat has been obſerved by Mr. SCHEELE; ſee his experiments on air and fire, p. 45. Engliſh tranſlation.

of the acid in the nitre is turned into phlogiſticated air, by abſorbing phlogiſton from the watery part.

From what has been ſaid it appears, that there is a conſiderable difference in the manner in which the acid acts in the production of dephlogiſticated air from red precipitate and from nitre; in the former caſe the acid comes over firſt, leaving the remaining ſubſtance deprived of part of its phlogiſton; in the latter the dephlogiſticated air comes firſt, leaving the acid loaded with the phlogiſton of the water from which it was formed.

On diſtilling a mixture of quickſilver and oil of vitriol to dryneſs, part of the acid comes over, loaded with phlogiſton, in the form of volatile ſulphureous acid and vitriolic acid air; ſo that the remaining white maſs may be conſidered as conſiſting of quickſilver deprived of its phlogiſton, and united to a certain proportion of acid and water, or of plain quickſilver united to a certain proportion of acid and dephlogiſticated air. Accordingly on urging this white maſs with a more violent heat, the dephlogiſticated air comes over, and at the ſame time part of the quickſilver riſes in its metallic form, and alſo part of the white maſs, united in all probability to a greater proportion of acid than before, ſublimes; ſo that the rationale of the production of dephlogiſticated air from turbith mineral, and from red precipitate, are nearly ſimilar.

True turbith mineral conſiſts of the abovementioned white maſs, well waſhed with water, by which means it acquires a yellow colour, and contains much leſs acid than the unwaſhed maſs. Accordingly it ſeems likely, that on expoſing this to heat, leſs of it ſhould ſublime without being decompounded, and conſequently that more dephlogiſticated air ſhould be procured from it than from the unwaſhed maſs.

This is an inſtance, that the ſuperabundant vitriolic acid may, in ſome caſes, be better extracted from the baſe it is united to by water than by heat. Vitriolated tartar is another inſtance; for, if vitriolated tartar be mixed with oil of vitriol and expoſed even to a pretty ſtrong red heat, the maſs will be very acid; but, if this maſs is diſſolved in water, and evaporated, the cryſtals will be not ſenſibly ſo.

In all probability, the vitriolic acid acts in the ſame manner in the production of dephlogiſticated air from alum, as the nitrous does in its production from nitre; that is, the watery part comes over firſt in the form of dephlogiſticated air, leaving the acid charged with its phlogiſton. Whether this is alſo the caſe with regard to green and blue vitriol, or whether in them the acid does not rather act in the ſame manner as in turbith mineral, I cannot pretend to ſay, but I think the latter more likely.

There is another way by which dephlogiſticated air has been found to be produced in great quantities, namely, the growth of vegetables expoſed to the ſun or day-light; the rationale of which, in all probability, is, that plants, when aſſiſted by the light, deprive part of the water ſucked up by their roots of its phlogiſton, and turn it into dephlogiſticated air, while the phlogiſton unites to, and forms part of, the ſubſtance of the plant.

There are many circumſtances which ſhew, that light has a remarkable power in enabling one body to abſorb phlogiſton from another. Mr. SENEBIER has obſerved, that the green tincture procured from the leaves of vegetables by ſpirit of wine, quickly loſes its colour when expoſed to the ſun in a bottle not more than one third part full, but does not do ſo in the dark. or if the bottle is quite full of the tincture, or if the air in it is phlogiſticated; whence it is natural to conclude, that the light enables the dephlogiſticated part of the air to abſorb phlogiſton from the tincture; and this appears to be really the caſe, as I find that the air in the bottle is conſiderably phlogiſticated thereby. Dephlogiſticated ſpirit of nitre alſo acquires a yellow colour, and becomes phlogiſticated, by expoſure to the ſun's rays *; and I find on trial that the air in the bottle in which it is contained becomes dephlogiſticated, or, in other words, receives an increaſe of dephlogiſticated air, which ſhews that the change in the acid is not owing to the ſun's rays communicating phlogiſton to it, but to their enabling it to abſorb phlogiſton from the water contained in it, and thereby to produce dephlogiſticated air. Mr. SCHEELE alſo found, that the dark colour acquired by luna cornea on expoſure to the light, is owing to part of the ſilver being revived; and that gold, diſſolved in aqua regia and deprived by diſtillation of the nitrous and ſuperfluous marine acid, is revived by the ſame means; and there is the utmoſt reaſon to think, that, in both caſes, the revival of the metal is owing to its abſorbing phlogiſton from the water.

Vegetables ſeem to conſiſt almoſt intirely of fixed and phlogiſticated air, united to a large proportion of phlogiſton and ſome water, ſince by burning in the open air, in which their phlogiſton unites to the dephlogiſticated part of the atmoſphere and forms

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* If ſpirit of nitre is diſtilled with a very gentle heat, the part which comes over is high coloured and fuming, and that which remains behind is quite colourleſs, and fumes much leſs than other nitrous acid of the ſame ſtrength, and the fumes are colourleſs. This is called dephlogiſticated ſpirit of nitre, as it appears to be really deprived of phlogiſton by the proceſs. The manner of preparing it, as well as its property of regaining its yellow colour by expoſure to the light, is mentioned by Mr. SCHEELE in the Stockholm Memoirs, 1774.

water, they ſeem to be reduced almoſt intirely to water and thoſe two kinds of air. Now plants growing in water without earth, can receive nouriſhment only from the water and air, and muſt therefore in all probability abſorb their phlogiſton from the water. It is known alſo that plants growing in the dark do not thrive well, and grow in a very different manner from what they do when expoſed to the light.

From what has been ſaid, it ſeems likely that the uſe of light, in promoting the growth of plants and the production of dephlogiſticated air from them, is, that it enables them to abſorb phlogiſton from the water. To this it may perhaps be objected, that though plants do not thrive well in the dark, yet they do grow, and ſhould therefore, according to this hypotheſis, abſorb water from the atmoſphere, and yield dephlogiſticated air, which they have not been found to do. But we have no proof that they grew at all in any of thoſe caſes in which they were found not to yield dephlogiſticated air; for though they will grow in the dark, yet their vegetative powers may perhaps at firſt be intirely checked by it, eſpecially conſidering the unnatural ſituation in which they muſt be placed in ſuch experiments. Perhaps too plants growing in the dark may be able to abſorb phlogiſton from water not much impregnated with dephlogiſticated air, but not from water ſtrongly impregnated with it; and conſequently, when kept under water in the dark, may perhaps at firſt yield ſome dephlogiſticated air, which, inſtead of riſing to the ſurface, may be abſorbed by the water, and, before the water is ſo much impregnated as to ſuffer any to eſcape, the plant may ceaſe to vegetate, unleſs the water is changed. Unleſs therefore it could be ſhewn that plants growing in the dark, in water alone, will increaſe in ſize, without yielding dephlogiſticated air, and without the water becoming more impregnated with it than before, no objection can be drawn from thence.

Mr. SENEBIER finds, that plants yield much more dephlogiſticated air in diſtilled water impregnated with fixed air, than in plain diſtilled water, which is perfectly conformable to the abovementioned hypotheſis; for as fixed air is a principal conſtituent part of vegetable ſubſtances, it is reaſonable to ſuppoſe that the work of vegetation will go on better in water containing this ſubſtance, than in other water.

There are ſeveral memoirs of Mr. LAVOISIER publiſhed by the Academy of Sciences, in which he intirely diſcards phlogiſton, and explains thoſe phænomena which have been uſually attributed to the loſs or attraction of that ſubſtance, by the abſorption or expulſion of dephlogiſticated air; and as not only the foregoing experiments, but moſt other phænomena of nature, ſeem explicable as well, or nearly as well, upon this as upon the commonly believed principle of phlogiſton, it may be proper briefly to mention in what manner I would explain them on this principle, and why I have adhered to the other. In doing this, I ſhall not conform ſtrictly to his theory, but ſhall make ſuch additions and alterations as ſeem to ſuit it beſt to the phænomena; the more ſo, as the foregoing experiments may, perhaps, induce tne author himſelf to think ſome ſuch additions proper.

According to this hypotheſis, we muſt ſuppoſe, that water conſiſts of inflammable air united to dephlogiſticated air; that nitrous air, vitriolic acid air, and the phoſphoric acid, are alſo combinations of phlogiſticated air, ſulphur, and phoſphorus, with dephlogiſticated air; and that the two former, by a further addition of the ſame ſubſtance, are reduced to the common nitrous and vitriolic acids; that the metallic calces conſiſt of the metals themſelves united to the ſame ſubſtance, commonly, however, with a mixture of fixed air; that on expoſing the calces of the perfect metals to a ſufficient heat, all the dephlogiſticated air is driven off, and the calces are reſtored to their metallic form; but as the calces of the imperfect metals are vitrified by heat, inſtead of recovering the metallic form, it ſhould ſeem as if all the dephlogiſticated air could not be driven off from them by heat alone. In like manner, according to this hypotheſis, the rationale of the production of dephlogiſticated air from red precipitate is, that during the ſolution of the quickſilver in the acid and the ſubſequent calcination, the acid is decompounded, and quits part of its dephlogiſticated air to the quickſilver, whereby it comes over in the form of nitrous air, and leaves the quickſilver behind united to dephlogiſticated air, which, by a further increaſe of heat, is driven off, while the quickſilver re-aſſumes its metallic form. In procuring dephlogiſticated air from nitre, the acid is alſo decompounded; but with this difference, that it ſuffers ſome of its dephlogiſticated air to eſcape, while it remains united to the alkali itſelf, in the form of phlogiſticated nitrous acid. As to the production of dephlogiſticated air from plants, it may be ſaid, that vegetable ſubſtances conſiſt chiefly of various combinations of three different baſes, one of which, when united to dephlogiſticated air, forms water, another fixed air, and the third phlogiſticated air; and that by means of vegetation each of theſe ſubſtances are decompoſed, and yield their dephlogiſticated air; and that in burning they again acquire dephlogiſticated air, and are reſtored to their priſtine form.

It ſeems, therefore, from what has been ſaid, as if the phænomena of nature might be explained very well on this princiciple, without the help of phlogiſton; and indeed, as adding dephlogiſticated air to a body comes to the ſame thing as depriving it of its phlogiſton and adding water to it, and as there are, perhaps, no bodies entirely deſtitute of water, and as I know no way by which phlogiſton can be transferred from one body to another, without leaving it uncertain whether water is not at the ſame time transferred, it will be very difficult to determine by experiment which of theſe opinions is the trueſt; but as the commonly received principle of phlogiſton explains all phænomena, at leaſt as well as Mr. LAVOISIER'S, I have adhered to that. There is one circumſtance alſo, which though it may appear to many not to have much force, I own has ſome weight with me; it is, that as plants ſeem to draw their nouriſhment almoſt intirely from water and fixed and phlogiſticated air, and are reſtored back to thoſe ſubſtances by burning, it ſeems reaſonable to conclude, that notwithſtanding their infinite variety they conſiſt almoſt intirely of various combinations of water and fixed and phlogiſticated air, united according to one of theſe opinions to phlogiſton, and deprived according to the other of dephlogiſticated air; ſo that, according to the latter opinion, the ſubſtance of a plant is leſs compounded than a mixture of thoſe bodies into which it is reſolved by burning; and it is more reaſonable to look for great variety in the more compound than in the more ſimple ſubſtance.

Another thing which Mr. LAVOISIER endeavours to prove is, that dephlogiſticated air is the acidifying principle. From what has been explained it appears, that this is no more than ſaying, that acids loſe their acidity by uniting to phlogiſton, which with regard to the nitrous, vitriolic, phoſphoric, and arſenical acids is certainly true. The ſame thing, I believe, may be ſaid of the acid of ſugar; and Mr. LAVOISIER'S experiment is a ſtrong confirmation of BERGMAN's opinion, that none of the ſpirit of nitre enters into the compoſition of the acid, but that it only ſerves to deprive the ſugar of part of its phlogiſton. But as to the marine acid and acid of tartar, it does not appear that they are capable of loſing their acidity by any union with phlogiſton. It is to be remarked alſo, that the acids of ſugar and tartar, and in all probability almoſt all the vegetable and animal acids, are by burning reduced to fixed and phlogiſticated air, and water, and therefore contain more phlogiſton, or leſs dephlogiſticated air, than thoſe three ſubſtances.

XIV. *Remarks on Mr.* Cavendiſh's *Experiments on Air. In a Letter from* Richard Kirwan, *Eſq. F. R. S. to Sir* Joſeph Banks, *Bart. P. R. S.*

Read Feb. 5, 1784.

SIR,

HAVING liſtened with much attention, and derived much uſeful information from the very curious experiments of Mr. CAVENDISH, read at our laſt meeting, it is with peculiar regret I feel myſelf withheld from yielding an intire aſſent to all he has advanced in his very ingenious paper; and it is with ſtill greater that I find myſelf obliged, by reaſon of the oppoſition of ſome of his deductions to thoſe I had the honour to lay before the ſociety about two years ago, to expoſe the reaſons of my diſſent, through your mediation, before this meeting.

In the paper already mentioned, read in April, 1782, I attributed the diminution of reſpirable air, obſerved in common phlogiſtic proceſſes, to the generation and abſorption of fixed air, which is now known to be an acid, and capable of being abſorbed by ſeveral ſubſtances. That fixed air was ſome how or other produced in phlogiſtic proceſſes, either by *ſeparation* or *compoſition*, I took for granted from the numerous experiments of Dr. PRIESTLEY; and among theſe I ſelected, as leaſt liable to objection, the Calcination of Metals, the decompoſition

tion of nitrous by mixture with reſpirable air, the phlogiſtication of reſpirable air by the electric ſpark, and, laſtly, that effected by amalgamation. In each of theſe inſtances Mr. CAVENDISH is of opinion, that the diminution of reſpirable air is owing to the production of water, which, according to him, is formed by the union of the phlogiſton, diſengaged in thoſe proceſſes, with the dephlogiſticated part of common air; and that fixed air is never produced in phlogiſtic proceſſes, except ſome animal or vegetable ſubſtance is concerned in the operation, from whoſe decompoſition it may ariſe. To which of theſe cauſes the diminution of reſpirable air is to be attributed, I ſhall now endeavour to elucidate.

Of the Calcination of Metals.

I attributed the diminution of air by the calcination of metals, to the converſion of the dephlogiſticated part of common air into fixed air, by reaſon of its union with the phlogiſton of the metal, for this plain reaſon, becauſe I find it acknowledged on all hands, that the calces of all the baſe metals yield fixed air, when ſufficiently heated. Mr. Cavendiſh allows the fact in general, but aſcribes the fixed air found in them to their long expoſure to the atmoſphere, in which he ſays fixed air pre-exiſts; but that it exiſts in common air in any quantity worth attending to, or is extracted from it in any degree, I take the liberty of denying, grounded on the following facts. Firſt, I have frequently agitated 18 cubic inches of common air in 2 of lime-water, and 2 of common air in 18 of lime-water, but could never perceive the ſlighteſt milkineſs; and yet the thouſandth part of a cubic inch of fixed air would thus be made ſenſible; for if a cubic inch of it be diſſolved in 3 ounces of

water,

water, a few drops of that water let into lime-water will produce a cloud. Mr. FONTANA ſays, he frequently agitated 1 cubic inch of Tincture of Turnſole in 7 or 800 of common air, without reddening it (23 Roz. p. 188.); and yet, according to Mr. BERGMAN, 1 cubic inch of fixed air is ſufficient to redden 50 of Tincture of Turnſole (1 BERGM. 11.); from whence I am apt to think, that 700 cubic inches of common air do not even contain $\frac{1}{10}$th of a cubic inch of fixed air. Dr. WHYTT found that 12 ounces of ſtrong lime-water, being expoſed to the open air for 19 days, ſtill retained about 1 grain of lime, (on Lime-water, p. 32.). Now 12 ounces of ſtrong lime-water contain at moſt 9,5 grains of lime, and 1 grain of lime requires only 0,56 of a cubic inch of fixed air to precipitate it, the thermometer at 55 and the barometer at 29,5, as I have found. Therefore in 19 days this lime-water did not come in contact with more than four cubic inches of fixed air; yet it is certain that a large quantity of fixed air is continually diſengaged, and thrown into the atmoſphere, by various proceſſes, as putrefaction, combuſtion, &c. but it ſeems equally certain that it is either decompoſed, or more probably abſorbed by various bodies. Mr. FONTANA let looſe 20000 cubic inches of fixed air, in a room whoſe windows and doors were cloſed, yet in half an hour after he could not diſcover the leaſt trace of it (ibid.). Though fixed air perpetually oozes from the floor of the *Grotto del Cane*, yet at the diſtance of four or five feet from the ground none is found; animals may live, lights burn, &c. (Roz. Ibid. Mem. Stockh. 1775.). If diſtilled water be expoſed to the atmoſphere, it is never found to abſorb fixed air, but rather dephlogiſticated air, according to Mr. SCHEELE's experiments, which could never happen if the atmoſphere contained any ſenſible proportion of fixed air; nor has rain-water been ever found to contain any, which it certainly ſhould on the ſame hypotheſis; even Mr. CAVENDISH himſelf could find no fixed air in the reſiduum or products of about 1040 ounce meaſures of common air, which he burnt with inflammable air.

It is true, Dr. PRIESTLEY ſuppoſed common air to contain $\frac{1}{55}$ of its bulk of fixed air; but he drew this concluſion not from any direct experiment, but from the quantity of fixed air produced by breathing, which he at that time believed to have been barely precipitated, and not generated, an opinion which he has found reaſon to alter from his own experiments. I think I may therefore conclude, that the quantity of fixed air contained in the atmoſphere is abſolutely inappreciable.

Secondly, ſuppoſing the atmoſphere to contain a very ſmall proportion of fixed air, yet I do not think it can be inferred that metals, during their calcination, extract any, becauſe I find that lime expoſed to red heat ever ſo long extracts none, though it is formed by a calcination in open air, which laſts at leaſt as long as that of any metal; neither does precipitate *per ſe* attract any, though its calcination laſts ſeveral months; nor does this proceed from the want of affinity, for if a ſaturate ſolution of mercury in any of the acids be precipitated by a mild vegetable alkali, very little effervеſcence is perceived, and the precipitate weighs much more than the quantity of mercury employed, and that this increaſe of weight ariſes in part from the fixed air abſorbed will preſently be ſeen.

Since then metals may be calcined in cloſe veſſels, ſince they then abſorb one fourth part of the common air to which they are expoſed, ſince all metallic calces (except thoſe of mercury, which I ſhall preſently mention) yield fixed air, ſince common air contains ſcarce any fixed air; is it not apparent that the fixed air thus found was generated by the very act of calcination, by the union of the phlogiſton of the metal with the dephlogiſticated part of the common air, ſince after the operation the metal is deprived of its phlogiſton, and the air of its dephlogiſticated part?

But Mr. CAVENDISH objects, that no one has extracted fixed air from metals calcined in cloſe veſſels. To which I anſwer, that this further proof is difficult, and no way neceſſary; it is difficult, becauſe the operation can eaſily be performed only on ſmall quantities; it is unneceſſary, becauſe it differs from the operation in open air only by the quantities of the materials employed, in every other reſpect it is exactly the ſame. Since Mr. CAVENDISH ſuſpects the reſults are different, it is incumbent on him to ſhew that difference; but until then, according to Sir ISAAC NEWTON's ſecond rule, *to natural effects of the ſame kind the ſame cauſes are to be aſſigned, as far as it may be done*, that is, until experience points out ſome other cauſe.

It may further be urged, that precipitate *per ſe* yields only dephlogiſticated air, that minium alſo yields a large proportion of it. This difficulty I have formerly anſwered by aſſerting, that theſe calces are in fact united only to fixed air, and that they yield dephlogiſticated air, merely becauſe the fixed air is decompoſed by the total or partial revivification of the metallic ſubſtances; this I think may be demonſtrated by the following experiments. Let ſublimate corroſive ſingly be treated in any manner, it will not yield dephlogiſticated air (4 Pr. 240.); but let a ſolution of ſublimate corroſive be precipitated by a mild fixed alkali, this precipitate waſhed, dried, and diſtilled in a pneumatic apparatus, will yield dephlogiſticated air, and the mercury will be revived; but, if the ſolution of ſublimate corroſive be precipitated by lime-water, it ſeems no air will be produced. Here then we ſee, 1ſt, that the calx of mercury unites with fixed air; and, 2dly, that this fixed air is, during the revivification of the mercury, converted into dephlogiſticated air. Again: let one ounce of red precipitate, which, according to Mr. CAVENDISH, contains no nitrous acid, be diſtilled with two ounces of filings of iron; this quantity of precipitate, which, if diſtilled by itſelf, would yield 60 ounce meaſures of dephlogiſticated air, will, when diſtilled with this proportion of filings of iron, yield 40 ounce meaſures of fixed air, as Dr. PRIESTLEY has ſhewn in his laſt paper: whichever way this is explained, ſome or other of my opinions are confirmed; for either the mercurial calx is already combined with fixed air (which I believe to be the caſe), and this air paſſes undecompoſed, becauſe the mercury extracts phlogiſton from the iron; or it contains dephlogiſticated air, which is converted into fixed air by its union with the phlogiſton of the iron.

If precipitate *per ſe* be digeſted in marine acid, the mercury will be revived (3 BERGM. 415.). Now this calx does not dephlogiſticate the marine acid; for this acid, when dephlogiſticated, diſſolves mercury; how then does it revive it, if not by expelling the fixed air contained in it, which in the moment of its expulſion is decompoſed, leaving its phlogiſton to the mercury, which is thereby revived?

Again: if litharge be heated in a gun-barrel, it will afford more fixed and leſs dephlogiſticated air than if heated in glaſs or earthen veſſels. Does not this happen, becauſe the calx of lead, receiving ſome phlogiſton from the metal, does not dephlogiſticate ſo great a proportion of the fixed air as it otherwiſe would?

Further: there is no ſubſtance which yields dephlogiſticated air, but yields alſo fixed air, even precipitate *per ſe* not excepted; (3 PRIEST. 16.) and what is remarkable, they all yield fixed air firſt, and dephlogiſticated air only towards the end of the proceſs. Does not this happen becauſe metallic calces attract phlogiſton ſo much more ſtrongly, as they are more heated? Thus many calciform iron ores become magnetic by calcination, though they were not ſo before; ſo alſo do all the calces of iron when expoſed to the focus of a burning glaſs (5 Dict. Chy. 179). Thus mercury cannot be calcined but in a heat inferior to that in which it boils; thus minium cannot be formed but in a moderate heat, and if heated ſtill more it returns to the ſtate of maſſicot, in which it was before it became minium, and much of it is reduced. So if a ſolution of luna cornea in volatile alkali be triturated with mercury, the ſilver will be revived, and the marine acid unite to the mercury, which ſhews this acid has a ſtronger attraction to Mercury than to ſilver; yet if ſublimate corroſive and ſilver be diſtilled in a ſtrong heat, the mercury will be revived, and the marine acid unite to the ſilver, which ſhews that the attraction of mercury to phlogiſton increaſes with the heat applied.

Before I conclude this head, I will mention another experiment, which I think deciſive in favour of my opinion of the compoſition of fixed air. If filings of zinc be digeſted in a cauſtic fixed alkali in a gentle heat, the zinc will be diſſolved with effervefcence, and the alkali will be rendered in great meaſure mild. But if, inſtead of filings of zinc, flowers of zinc be uſed, and treated in the ſame manner, there will be no ſolution, and the alkali will remain cauſtic. In the firſt caſe the effervefcence ariſes from the production of inflammable air, which phlogiſticates the common air contiguous to it, and produces fixed air, which is immediately abſorbed by the alkali, and renders it mild. In the ſecond caſe, no inflammable air is produced, the common air is not phlogiſticated, and conſequently the alkali remains cauſtic*. This experiment alſo proves that metallic calces attract fixed air more ſtrongly than alkalies attract it; for the calces of zinc are known to contain fixed air, and yet alkalies digeſted with them remain cauſtic; and this accounts for the ſlight turbidity of lime-water when metals are calcined over it; for as ſoon as the phlogiſton is diſengaged from the metal, and before it has abſorbed the whole quantity of fire requiſite to throw it into the form of inflammable air, it meets with the dephlogiſticated part of the common air on the ſurface of the metal, and there forms fixed air, which is inſtantly abſorbed by the calx with which it is in contact, ſo that it is not to be wondered that it does not unite to the lime from which it is diſtant.

Of the Decompoſition of Nitrous Air by mixture with Common Air.

AS ſoon as I had heard Mr. CAVENDISH's paper read, I ſet about trying whether lime would be precipitated from lime-water during the proceſs, an experiment I had never made before with common air, taking it for granted that it was ſo, from the repeated experiments of Dr. PRIESTLEY, and indeed of all others who had treated this ſubject †: and, in effect, when I made the experiment with nitrous air prepared and confined by the water of my tub, I found lime-water admitted to it inſtantly precipitated. But after I had read Mr. CAVENDISH's paper, which he had the politeneſs to permit me, and had, according to his direction, received the nitrous air over lime-water, I did not then perceive the leaſt milkineſs after admitting common air. After 12 hours I indeed perceived a whitiſh duſt, on the bottom of the glaſs veſſel in which I made the experiment, which I cannot aſſure to be calcareous; and, on breathing into the lime-water, an evident milkineſs enſued; ſo that I little doubt but the precipitation I obſerved in the firſt experiment aroſe from the decompoſition of the aerial ſelenite contained in the water of the tub. And it is very poſſible that the precipitation of lime, which I perceived ſome years ago on mixing dephlogiſticated air and nitrous air, might have ariſen from the ſame cauſe, or from fixed air pre-contained in the dephlogiſticated, as this laſt had not been waſhed in lime-water. Yet I do not think the failure of this experiment at all concluſive againſt the ſuppoſed production of fixed air on this occaſion, becauſe the quantity of fixed air is ſo ſmall, that it may well be ſuppoſed to unite to the nitrous ſelenite formed in the lime-water. It is well known that a ſmall quantity of fixed air is capable of uniting to all neutral ſalts: thus Dr. PRIESTLEY has extracted it from tartar vitriolate and alum, (2 PR. 115, 116.) and gypſum, (2 PR. 80.); and Dr. MAC BRIDE found it in nitre and common ſalt, though in ſmall quantity. But to try whether nitrous ſelenite would attract any, I made a ſolution of chalk in nitrous acid, which, when ſaturate, weighed 381,25 grains; but, being expoſed to the air for a few hours, it weighed 382,25. I afterwards took a very dilute nitrous acid, in which an acid taſte was barely perceptible, and impregnated it with a very ſmall proportion

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* See Mr. LASSONE's Experiments on zinc. Mem. Par. 1777. p. 7 & 8.

† See 1 Pr. 114, 189. 2 Pr. 218. Font. Recherches Phyſ. p. 77. 1 Chy. Dij. 324.

portion of fixed air, and then let fall a few drops of it into lime-water; not the ſmalleſt cloud was perceived, and yet when I breathed into it afterwards it became milky in a few ſeconds; ſo that this experiment is perfectly analogous to that in which nitrous and common air were mixed.

But if nitrous air and common air be mixed over dry mercury, the reſult is intirely adverſe to the opinion of Mr. CAVENDISH, and favourable to mine; for in this caſe the common air is not at all diminiſhed until water is admitted to it, and the mixture agitated a few minutes, and then the diminution is nearly the ſame as if the mixture were made over water. Thus when I mixed two cubic inches of common air with one of nitrous air, they occupied the ſpace of two inches and one-eighth, and the ſurface of the mercury was immediately calcined; which ſhews that the inch of nitrous air was decompoſed, and produced nitrous acid; but the common air was undiminiſhed; and the one eighth of an inch over and above the two inches of common air, proceeded from an addition of new nitrous air, formed by the corroſion of the ſurface of the mercury. That the common air ſhould remain undiminiſhed is eaſily explained in my ſyſtem, becauſe fixed air is formed, which, on this occaſion, muſt remain unabſorbed, at leaſt for a long time, as there is nothing at hand that can immediately receive it; and hence, if water be admitted ſoon after the mixture of both airs, the diminution will be nearly the ſame as if the mixture had been originally made over water, though not exactly the ſame; becauſe the nitrous air, produced by the union of the newly formed nitrous acid with the mercury, is not entirely abſorbable by water. But, in Mr. CAVENDISH's hypotheſis, the common air ſhould be diminiſhed juſt as much as if the mixture were made over water; for, according to him, this diminution ariſes from the converſion of the dephlogiſticated part of the common air into water, which water ſhould immediately unite to the nitrous ſalt of mercury, and leave the common air leſſened in its bulk by a portion commenſurate to that converted into water, or, if he will not allow the water to have immediately united to the mercurial ſalt, at leaſt by the difference of the bulk of the water produced, and that of an equal weight of the common air converted into it: but neither happens; for the common air is not at all diminiſhed; nor can he explain, conſiſtently with his ſyſtem, why the admiſſion of water ſhould immediately produce a diminution in the common air, as, according to him, it contains nothing that can be abſorbed. Dr. PRIESTLEY has remarked, that if a mixture of both airs be ſuffered to ſtand ſeveral hours, even the admiſſion of water will produce no diminution. This is owing to two cauſes; 1ſt, becauſe a large quantity of nitrous air is produced, by the continued action of the concentrated nitrous acid newly formed; and, 2dly, becauſe the fixed air, on whoſe abſorption the diminution depends, is abſorbed by the mercurial ſalt, as may be inferred from the experiment in 1 LAVOISIER, p. 248.

Of the Diminution of Common Air by the Electric Spark.

Of all the inſtances of the artificial production of fixed air, by the union of phlogiſton with the dephlogiſticated part of common air, there is none perhaps ſo convincing, as that exhibited by taking the electric ſpark through common air, over a ſolution of litmus, or lime-water; for the common air is diminiſhed one fourth, the litmus reddened, and the lime-water precipitated. Mr. CAVENDISH indeed attributes the redneſs of the litmus to fixed air; but he thinks it proceeds from a decompoſition of ſome part of the vegetable juice, as all vegetable juices contain fixed air. Yet that ſuch a decompoſition does not take place, I think may be inferred from the following reaſons: firſt, if the electric ſpark be taken through phlogiſticated or inflammable air confined by litmus, no redneſs is produced, the air not being in the leaſt diminiſhed; and, 2dly, if the litmus were decompoſed, inflammable air ſhould be produced as well as fixed air; and then there ſhould be an addition of bulk inſtead of a diminution; but what ſets the origin of the fixed air from the phlogiſtication of the common air beyond all doubt is, that if lime-water be uſed inſtead of litmus, the diminution is the ſame, and the lime is precipitated. Here Mr. CAVENDISH ſays, the fixed air proceeds either from *ſome dirt in the tube*; a ſuppoſition, which, being neither neceſſary nor probable, is not admiſſible; or elſe *from ſome combuſtible matter in the lime*; but lime contains no combuſtible matter, except perhaps phlogiſton, which cannot produce fixed air but by uniting to the common air, according to my ſuppoſition; but it is much more probable, that the diminution does not ariſe from any phlogiſton in the lime, as it is exactly the ſame whether lime-water be uſed or not; and the lime does not appear to be in the leaſt altered, and in fact contains ſcarce any phlogiſton.

Of the diminution of Common Air, by the Amalgamation of Mercury and Lead.

I attributed this diminution to the phlogiſtication of the common air by the proceſs of amalgamation, and the conſequent production and abſorption of fixed air. On this Mr. CA-

VENDISH obſerves, " that mercury, fouled by the addition of
" lead or tin, depoſits a powder which conſiſts in great meaſure
" of the calx of the metal: he found alſo, that ſome powder of
" this ſort contained fixed air; but it is not clear that this air
" was produced by the phlogiſtication of the air in which the
" mercury was ſhaken, as the powder was not prepared on
" purpoſe, but was formed from mercury fouled by having
" been uſed for various purpoſes, and may therefore contain
" other impurities, beſides the metallic calx." On this I remark, that Dr. PRIESTLEY did not indeed at firſt prepare this powder on purpoſe; but he afterwards did ſo prepare it (4 PRIEST. p. 148, 149.) and obtained a powder exactly of the ſame ſort; and it is certain that the fixed air found in it proceeded from the common air, both becauſe metallic calces, not formed by amalgamation, will not unite with mercury, as is well known; and becauſe this calx cannot be formed by agitation of the mercury and lead, in phlogiſticated, inflammable, or any other air which is not reſpirable; and the fixed air cannot proceed from any impurity, as mercury will not unite in its running form to any other but metallic ſubſtances, which it always partially dephlogiſticates, like other menſtruums (3 Chy. Dijon, 425.).

Of the Diminution of Reſpirable Air by Combuſtion.

Though I have no doubt but the diminution of reſpirable air, by the combuſtion of ſulphur and phoſphorus, proceeds alſo in great meaſure from the production and abſorption of fixed air, yet I avoided mentioning this operation, as the preſence of a ſtronger acid renders the preſence of a weaker impoſſible to be proved, more eſpecially, as both theſe acids precipitate lime from lime-water; but the great increaſe of weight which the phoſphoric acid gains is a ſtrong additional inducement to think that it abſorbs fixed air. During the combuſtion of vegetable ſubſtances, I think it highly probable that fixed air is formed, both from my own experiments on the combuſtion of wax candles, and that mentioned in the firſt volume of Dr. PRIESTLEY'S Obſervations, p. 136; but when inflammable air from metals and dephlogiſticated air are fired, as a great diminution takes place, and yet no fixed air is found, I am nearly convinced, by Mr. CAVENDISH'S experiments, that water is really produced; nor am I ſurprized that, in this inſtance, the union of phlogiſton and dephlogiſticated air ſhould form a compound very different from that which it forms in other inſtances of phlogiſtication, but ſhould rather be led to expect it *a priori*; for in this caſe the phlogiſton is in its moſt rarefied known ſtate, and unites to dephlogiſticated air, the ſubſtance to which it has the greateſt affinity, in circumſtances the moſt favourable to the cloſeſt and moſt intimate union; for both, in the act of inflammation, are rarefied to the higheſt degree; both give out their ſpecific fire, the great obſtacle to their union, it being by the inflammation converted into *ſenſible* heat (a circumſtance which, in my opinion, conſtitutes the very eſſence of flame); the reſulting compound having then loſt the greateſt part of its ſpecific fire, is neceſſarily reduced, according to Dr. BLACK'S theory, into a denſer ſtate, which the preſent experiment ſhews to be water; whereas, in common caſes of combuſtion, the phlogiſton being denſer and leſs divided, unites leſs intimately with the dephlogiſticated part of common air, conſequently expels leſs of its ſpecific fire, and therefore forms leſs denſe compounds, *viz.* fixed and phlogiſticated airs; and ſo much the more, as a great part intirely eſcapes combuſtion; but it ſeems probable that in very ſtrong and bright inflammations, the union is more perfect, and water formed.

Water being then the reſult of the cloſeſt and moſt intimate union of dephlogiſticated air and phlogiſton, it ſeems to me very improbable, that it is ever decompoſed by the affinity of any acid to phlogiſton, as all the experiments hitherto made ſeem to prove, that phlogiſton has a ſtronger affinity to dephlogiſticated air than to any other ſubſtance, except hot metallic calces; and theſe, in my opinion, are incapable of forming any union with water, except as far as they are ſaline, but they never can be reduced by it. So alſo water is incapable of uniting with any more phlogiſton, as ſulphur is, both being already ſaturated.

Mr. Cavendish is inclined to think, that pure inflammable air is not pure phlogiſton, becauſe it does not immediately unite with dephlogiſticated air, when both airs are ſimply mixed with each other; this reaſon ſeems to me of no moment, becauſe I ſee ſeveral other ſubſtances, that have the ſtrongeſt affinity to each other, refuſe to unite ſuddenly, or even at all, through the very ſame cauſe that dephlogiſticated and inflammable airs refuſe to unite; *viz.* on account of the ſpecific fire which they contain, and muſt loſe, before ſuch union can take place: thus fixed air will never unite to dry lime, though they be kept ever ſo long together; thus, if water be poured on the ſtrongeſt oil of vitriol, they will remain ſeveral weeks in contact, without uniting, as I myſelf have experienced; and yet, in both caſes, the ſpecific fire need be expelled only from one of the ſubſtances, and not from both: but after a long time they will unite; ſo alſo will inflammable and dephlogiſticated air, as Dr. Priestley has diſcovered ſince his laſt publication.

That phlogiſticated air ſhould conſiſt of ſuperſaturated nitrous air, I think improbable, as it retains its phlogiſton much more ſtrongly than nitrous air, which, according to the general laws of affinities, it ſhould not, if it contained an exceſs of phlogiſton; and as Dr. PRIESTLEY and Mr. FONTANA repeatedly aſſure us, they have converted it into common air, by waſhing it in water, in contact with the atmoſphere. I am, *&c.*

London,
Jan. 29, 1784.

R. KIRWAN.

XV. *Anſwer to Mr.* Kirwan's *Remarks upon the Experiments on Air. By* Henry Cavendiſh, *Eſq. F. R. S. and S. A.*

Read March 4, 1784.

IN a paper lately read before this Society, containing many experiments on air, I gave my reaſons for ſuppoſing that the diminution which reſpirable air ſuffers by phlogiſtication, is not owing either to the generation or ſeparation of fixed air from it; but without any arguments of a perſonal nature, or which related to any one perſon who eſpouſes the contrary doctrine more than to another. This being contrary to the opinion maintained by Mr. KIRWAN, he has written a paper in anſwer to it, which was read on the fifth of February. As I do not like troubling the Society with controverſy, I ſhall take no notice of the arguments uſed by him, but ſhall leave them for the reader to form his own judgement of; much leſs will I endeavour to point out any inconſiſtencies or falſe reaſonings, ſhould any ſuch have crept into it; but as there are two or three experiments mentioned there, which may perhaps be conſidered as diſagreeing with my opinion, I beg leave to ſay a few words concerning them.

Mr. DE LASSONE found that filings of zinc, digeſted in a cauſtic fixed alkali, were partially diſſolved with a ſmall effervescence, and that the alkali was rendered in ſome meaſure mild. This mildneſs of the alkali Mr. KIRWAN accounts for by ſuppoſing, that the inflammable air, which is ſeparated during the ſolution, and cauſes the efferveſcence, unites to the atmoſpheric air contiguous to it, and thereby generates fixed air, which is abſorbed by the alkali. But, in reality, the only circumſtance from which Mr. DE LASSONE judged the alkali to become mild, was its making ſome efferveſcence when ſaturated with acids; and this efferveſcence is more likely to have proceeded from the expulſion of inflammable air than of fixed air, as it ſeems likely, that the zinc might be more completely deprived of its phlogiſton by the acid than by the alkali.

In the abovementioned paper I ſay, Dr. PRIESTLEY obſerved, that quickſilver fouled by the addition of lead or tin, depoſits a powder by agitation and expoſure to the air, which conſiſts in great meaſure of the calx of the imperfect metal. He found too ſome powder of this kind to contain fixed air; but it muſt be obſerved, that the powder uſed in this experiment was not prepared on purpoſe, but was procured from quickſilver fouled by having been uſed in various experiments, and may therefore have contained other impurities beſides the metallic calces. On this Mr. KIRWAN remarks, that Dr. PRIESTLEY did not at firſt prepare this powder on purpoſe, but he afterwards did ſo prepare it (4 PR. p. 148. and 149.), and obtained a powder exactly of the ſame ſort. It was natural to ſuppoſe from this remark, that Dr. PRIESTLEY muſt have obtained fixed air from the powder prepared on purpoſe, and that I had overlooked the paſſage; but, on turning to the pages referred to, I was ſurpriſed to find that it was otherwiſe, and that Dr. PRIESTLEY not ſo much as hints that he procured fixed air from the powder thus prepared.

With regard to the calcination of metals it may be proper to remark, that this operation is uſually performed over the fire, by methods in which they are expoſed to the fumes of the burning fuel, and which are ſo replete with fixed air, that it is not extraordinary, that the metallic calx ſhould, in a ſhort time, abſorb a conſiderable quantity of it; and in particular red lead, which is the calx on which moſt experiments have been made, is always ſo prepared. There is another kind of calcination, however, called ruſting, which is performed in the open air; but this is ſo ſlow an operation, that the ruſt may eaſily imbibe a ſufficient quantity of fixed air, notwithſtanding the ſmall quantity of it uſually contained in the atmoſphere.

Mr. KIRWAN allows that lime-water is not rendered cloudy by the mixture of nitrous and common air; but contends that this does not prove that fixed air is not generated by the union, as he thinks it may be abſorbed by the nitrous ſelenite produced by the union of the nitrous acid with the lime. This induced me to try how ſmall a quantity of fixed air would be perceived in this experiment. I accordingly repeated it in the ſame manner as deſcribed in my paper, except that I purpoſely added a little fixed air to the common air, and found that when this addition was $\frac{1}{75}$th of the bulk, or $\frac{1}{50}$th of the weight of the common air, the effect on the lime-water was ſuch as could not poſſibly have been overlooked in my experiments. But as thoſe who ſuppoſe fixed air to be generated by the mixture of nitrous and common air, may object to this manner of trying the experiment, and ſay, that the quantity of fixed air abſorbed by the lime-water was really more than $\frac{1}{75}$th of the bulk of the common air, being equal to that quantity over and above the air generated by the mixture, I made another experiment in a different manner; namely, I filled a bottle with lime-water, previouſly mixed with as much nitrous acid as is contained in an equal bulk of nitrous air, and having inverted it into a veſſel of the ſame, let up into it, in the ſame manner as in the above-mentioned experiments, a mixture of common air with $\frac{1}{75}$th of its bulk of fixed air, until it was half full. The event was the ſame as before; namely, the cloudineſs produced in the lime-water was ſuch that I could not poſſibly have overlooked. It muſt be obſerved, that in this experiment no fixed air could be generated, and a ſtill greater proportion of the lime-water was turned into nitrous ſelenite than in the above-mentioned experiments; ſo that we may ſafely conclude, that if any fixed air is generated by the mixture of common and nitrous air, it muſt be leſs than $\frac{1}{75}$th of the bulk of the common air.

As for the nitrous ſelenite, it ſeems not to make the effect of the fixed air at all leſs ſenſible, as I found by filling two bottles with common air mixed with $\frac{1}{100}$dth of its bulk of fixed air, and pouring into each of them equal quantities of diluted lime-water; one of theſe portions of lime-water being previouſly diluted with an equal quantity of diſtilled water, and the other with the ſame quantity of a diluted ſolution of nitrous ſelenite, containing about $\frac{1}{400}$dth of its weight of calcareous earth; when I could not perceive that the latter portion of lime-water was rendered at all leſs cloudy than the former. Though the nitrous ſelenite, however, does not make the effect of the fixed air leſs ſenſible, yet the dilution of the lime-water, in conſequence of ſome of the lime being abſorbed by the acid, does; but, I believe, not in any remarkable degree.

There is an experiment mentioned by Mr. KIRWAN which, though it cannot be conſidered as an argument in favour of the generation of fixed air, as he only ſuppoſes, without any proof, that fixed air is produced in it, does yet deſerve to be taken notice of as a curious experiment. It is, that, if nitrous and common air be mixed over dry quickſilver, the common air is not at all diminiſhed, that is, the bulk of the mixture will be not leſs than that of the common air employed, until water is admitted, and the mixture agitated for a few minutes. The reaſon of this in all probability is, that part of the phlogiſticated nitrous acid, into which the nitrous air is converted, remains in the ſtate of vapour until condenſed by the addition of water. A proof that this is the real caſe is, that, in this manner of performing the experiment, the red fumes produced on mixing the airs remain viſible for ſome hours, but immediately diſappear on the addition of water and agitation.

The moſt material experiment alledged by Mr. KIRWAN is one of Dr. PRIESTLEY's, in which he obtained fixed air from a mixture of red precipitate and iron filings. This at firſt ſeems really a ſtrong argument in favour of the generation of fixed air; for though plumbago, which is known to conſiſt chiefly of that ſubſtance, has lately been found to be contained in iron, yet one would not have expected it to be decompounded by the red precipitate, eſpecially when the quantity of pure iron in the filings was much more than ſufficient to ſupply the precipitate with phlogiſton. The following experiment, however, ſhews that it was really decompounded; and that the fixed air obtained was not generated, but only ſeparated by means of this decompoſition.

500 grains of red precipitate mixed with 1000 of iron filings yielded, by the aſſiſtance of heat, 7800 grain meaſures of fixed

air, beſides 2400 of a mixture of dephlogiſticated and inflammable air, but chiefly the latter. The ſame quantity of iron filings, taken from the ſame parcel, was then diſſolved in diluted oil of vitriol, ſo as to leave only the plumbago and other impurities. Theſe mixed with 500 grains of the ſame red precipitate, and treated as before, yielded 9200 grain meaſures of fixed air, and 4200 of dephlogiſticated air, of an indifferent quality, but without any ſenſible mixture of inflammable air. It appears, therefore, that leſs fixed air was produced when the red precipitate was mixed with the iron filings in ſubſtance, than when mixed only with the plumbago and other impurities; which ſhews, that its production was not owing to the iron itſelf, which ſeems to contain no fixed air, but to the plumbago, which contains a great deal. The reaſon, in all probability, why leſs fixed air was produced in the firſt caſe than the latter is, that in the former more of the plumbago eſcaped being decompounded by the red precipitate than in the other. It muſt be obſerved, however, that the filings uſed in this experiment were mixed with about $\frac{1}{13}$th of their weight of braſs, which was not diſcovered till they were diſſolved in the acid, and which makes the experiment leſs deciſive than it would otherwiſe be. The quantity of fixed air obtained is alſo much greater than, according to Mr. BERGMAN's experiment, could be yielded by the plumbago uſually contained in 1000 grains of iron; ſo that though the experiment ſeems to ſhew that the fixed air was only produced by the decompoſition of the impurities in the filings, yet it certainly ought to be repeated in a more accurate manner.

Before I conclude this paper, it may be proper to ſum up the ſtate of the argument on this ſubject. There are five methods of phlogiſtication conſidered by me in my paper on air;

namely, firſt, the calcination of metals, either by themſelves or when amalgamated with quickſilver; ſecondly, the burning of ſulphur or phoſphorus; thirdly, the mixture of nitrous air; fourthly, the exploſion of inflammable air; and, fifthly, the electric ſpark; and Mr. KIRWAN has not pointed out any other which he conſiders as unexceptionable. Now the laſt of theſe I by no means conſider as unexceptionable, as it ſeems much moſt likely, that the phlogiſtication of the air in that experiment is owing to the burning or calcination of ſome ſubſtance contained in the apparatus *. It is true, that I have no proof of it; but there is ſo much probability in the opinion, that till it is proved to be erroneous, no concluſion can be drawn from ſuch experiments in favour of the generation of fixed air. As to the firſt method, or the calcination of metals, there is not the leaſt proof that any fixed air is generated, though we certainly have no direct proof of the contrary; nor did I in my paper inſinuate that we had. The ſame thing may be ſaid of the burning of ſulphur and phoſphorus. As to the mixture of nitrous air, and the combuſtion of inflammable air, it is proved, that if any fixed air is generated, it is ſo ſmall as to elude the niceſt teſt we have. It is certain too, that if it had been ſo much as $\frac{1}{70}$th of the bulk of the common air employed, it would have been perceived in the firſt of theſe methods, and would have been ſenſible in the ſecond though ſtill leſs. So that out of the five methods enumerated, it has been ſhewn, that in two no ſenſible quantity is generated, and not the leaſt proof has been aſſigned that any is in two of the

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* In the experiment with the litmus I attribute the fixed air to the burning of the litmus, not decompoſition, as Mr. KIRWAN repreſents it, which is a ſufficient reaſon why no fixed air ſhould be found when the experiment is tried with air in which bodies will not burn.

others; and as to the laſt, good reaſons have been aſſigned for thinking it inconcluſive; and therefore the concluſion drawn by me in the above-mentioned paper ſeems ſufficiently juſtified; namely, that though it is not impoſſible that fixed air may be generated in ſome chemical proceſſes, yet it ſeems certain, that it is not the general effect of phlogiſticating air, and that the diminution of common air by phlogiſtication is by no means owing to the generation or ſeparation of fixed air from it.

XVI. *Reply to Mr.* Cavendiſh's *Anſwer.*

By Richard Kirwan, *Eſq. F. R. S.*

Read March 18, 1784.

I MEAN to trouble the Society but with a very few words in reply to Mr. CAVENDISH's anſwer, as I conſider the greater part of mine to him as ſtill unanſwered.

In the firſt place, he ſays, that in Mr. LASSONE's experiment the effervеſcence proceeded not from any fixed air in the alkali, but from the further action of the acid on the zinc from which inflammable air was diſengaged. But this could not have happened; for, firſt, the zinc, inſtead of being further acted on by the acid, was precipitated according to Mr. LASSONE's own account (p. 8.); and, ſecondly, the acid was only added by degrees, and undoubtedly would unite to the alkali preferably to the zinc; therefore it was from the alkali, and not from the zinc, that the effervеſcence aroſe.

2dly, With regard to the calcination of lead; though in England the ſmoke and flame may come in contact with the metal, yet in Germany red lead is formed without any communication between them, according to Mr. NOSE, who has given an ample account of this manufactory (p. 86.). Is not lime formed in contact with fuel, flame, and ſmoke? Mr. MACQUER even thinks it probable, that the contact of flame is hurtful to the production of minium (2 Dict. Chy. 639.). Mr. MONNET made minium by melting lead in a cuppel, in ſuch a manner that it was impoſſible it could come in contact with the leaſt particle of flame or ſmoke (Mem. Turin. 1769, p. 71.).

Mr. CAVENDISH expreſſes his ſurpriſe at my aſſerting, that the black powder, which Dr. PRIESTLEY formed out of an amalgam of mercury and lead, was exactly the ſame as that out of which he had extracted fixed air; but, I think, I have aſſigned very ſufficient reaſons for my opinion: how far I was right will beſt appear by Dr. PRIESTLEY's own letter, in the hands of the Secretary, of which the following is an extract.

"I certainly imagined the two black powders you write "about to be of the ſame nature, and therefore did not at"tempt to extract any air from the latter; but immediately "on the receipt of your favour of yeſterday, I diſſolved an "ounce of lead in mercury, and expelling it by agitation, "put the black powder, which weighed near 12 ounces, into "a coated glaſs retort; then applying heat, I got from it about "20 ounce meaſures of very pure fixed air, not $\frac{1}{10}$th of which "remained unabſorbed by water."

Fourthly, it is impoſſible to attribute the fixed air, produced by the diſtillation of red precipitate and filings of iron, to the decompoſition of the plumbago contained in the iron; for the quantity of fixed air produced in Mr. CAVENDISH's own experiment is more than *twice* the weight of the whole quantity of plumbago contained in the quantity of iron he uſed, ſuppoſing the whole of the plumbago to to conſiſt of fixed air, which is not pretended; and more than *eight* times the weight of the quantity of fixed air which plumbago really contains. For Mr. CAVENDISH employed in his experiment 1000 grains of iron and 500 grains of red precipitate, and obtained 7800 grain meaſures of fixed air, which are equal to 30 cubic inches, and weigh 17 grains. Now 100 grains of bar iron contain, according to Mr. BERGMAN, at moſt, two-tenths of a grain of plumbago; and conſequently 1000 grs. of this iron contain but two grains of plumbago; and plumbago, according to Mr. SCHEELE, contains but one-third of its weight of fixed air; ſo that here, ſuppoſing the plumbago to be decompoſed, we can have at moſt but ſeven-tenths of a grain of fixed air, or little more than one cubic inch. If we ſuppoſe the filings to be from ſteel, 1000 grains of ſteel containing eight of plumbago, we may have about 2,5 of fixed air, or about 1,5 cubic inch, and this is the ſtrongeſt ſuppoſition, and the moſt favourable to Mr. CAVENDISH. What ſhall we then ſay, if we conſider that theſe filings were mixed with copper or braſs which contain no plumbago? and, above all, that plumbago cannot be ſuppoſed decompoſable by red precipitate, ſince even the nitrous acid cannot decompoſe it?

5thly, With regard to the power which nitrous ſelenite has of abſorbing fixed air, I muſt allow the experiments of Mr. CAVENDISH to be juſt and agreeable to my own; but it only follows, that when fixed air is in its *naſcent* ſtate, it is more abſorbable. Thus many metallic calces take it from alkalies in its *naſcent* ſtate, though in other circumſtances they will take none.

Laſtly, the permanence of a mixture of nitrous and common air, made over mercury, cannot be attributed to nitrous vapour, as vapour is not elaſtic in cold; beſides, I have often made the mixture without producing any ſuch durable vapour, and this will always happen, when the nitrous air is made from nitrous acid ſufficiently diluted.

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XVII. *On a Method of deſcribing the relative Poſitions and Magnitudes of the Fixed Stars; together with ſome Aſtronomical Obſervations. By the Rev.* Francis Wollaſton, *LL.B. F. R. S.*

Read February 5, 1784.

FROM ſome alterations which have of late years been diſcovered, in the relative poſitions and apparent magnitudes of a few of the ſtars we called fixed, it ſeems not unreaſonable to conclude, that there may be many changes among others of them we little ſuſpect. This thought has led me into a wiſh, that ſome method were adopted whereby to detect ſuch motions. The firſt idea which occurred to me was, to make a propoſal to aſtronomers in general; that each ſhould undertake a *ſtrict* examination of a certain diſtrict in the heavens; and, not only by a re-examination of the catalogues hitherto publiſhed, but by taking the right aſcenſion and declination of every ſtar in their ſeveral allotment, to frame an exact map of it, with a correſponding catalogue; and to communicate their obſervations to one common centre. This is what I could be glad to ſee begun. Every aſtronomer muſt wiſh it, and therefore every one ſhould be ready to take his ſhare in it. Such a plan, undertaken with ſpirit, and carried on gradually with care, would, by the joint labours and emulation of ſo many aſtronomers as are now in Europe, produce a celeſtial Atlas far beyond any thing that has ever yet appeared.

But this would be a work of time, and not within the compaſs of every one. What I mean now to propoſe is more immediate; and not out of the reach of any who amuſe themſelves with viewing the heavenly bodies.

Meridian altitudes and tranſits can be taken but once in 24 hours; and, though accurate, are therefore tedious. Neither can any re-examination of them be made, but with the ſame labour as at the firſt. Equatorial ſectors are in the hands of few; and require great ſkill. Some more general method ſeemed wanting; to diſcover variations, which, when detected or only ſurmiſed, ſhould be conſigned immediately to a more ſtrict inveſtigation.

Turning this in my thoughts, I conſidered, that the noting down at the time the exact appearance of what one ſees, would be far more ſimple, and ſhew any alterations in that appearance more readily, than any other method. A Drawing once made would remain, and could be conſulted at any future period; and if it were drawn at firſt with care, a tranſient review would diſcover to one, whether any ſenſible change had taken place ſince it was laſt examined. Catalogues, or verbal Deſcriptions of any kind, could not anſwer that end ſo well.

To do this with eaſe and expedition was then the requiſite: and a teleſcope with a large field, and ſome proper ſub-diviſions in it, to direct the eye and aſſiſt the judgement, ſeemed to bid moſt fair for ſucceſs.

The following is the method which, after various trials, I have adopted, and think I may now venture to recommend.

To a night-glaſs, but of DOLLOND's improved conſtruction, which magnifies about ſix times, and takes in a field of juſt about as many degrees of a great circle, I have added croſs wires, interſecting each other at an angle of 45°. More wires may be croſſed in other directions; but I apprehend theſe will be found ſufficient. This teleſcope I mount on a polar axis. One coarſely made, and without any diviſions on its circle of declination, will anſwer this purpoſe, ſince there is no great occaſion for accuracy in that reſpect: but as the heavenly bodies are more readily followed by an equatorial motion of the teleſcope, ſo their relative poſitions are much more eaſily diſcerned when they are looked at conſtantly as in the ſame direction. An horizontal motion, except in the meridian, would be apt to miſlead the judgement. It is ſcarcely neceſſary to add, that the wires muſt ſtand ſo as for one to deſcribe a parallel of the equator nearly. Another will then be a horary circle; and the whole area will be divided into eight equal ſectors.

Thus prepared, the teleſcope is to be pointed to a known ſtar, which is to be brought into the centre or common interſection of all the wires. The relative poſitions of ſuch other ſtars as appear within the field, are to be judged-of by the eye: whether at $\frac{1}{2}$, or $\frac{1}{3}$, or $\frac{1}{4}$ from the centre towards the circumference, or *vice verſâ*; and ſo with regard to the neareſt wire reſpectively. Theſe, as one ſees them, are to be noted down with a black-lead pencil upon a large meſſage card held in the hand, upon which a circle, ſimilarly divided, is ready drawn. (One of three inches diameter ſeems moſt convenient.) The motion of the heavenly bodies in ſuch a teleſcope is ſo ſlow, and the noting down of the ſtars ſo quickly done, that there is moſt commonly full time for it without moving the teleſcope. When that is wanted, the principal ſtar is eaſily brought back again into the centre of the field at pleaſure, and the work reſumed. After a little practice, it is aſtoniſhing how near one can come to the truth in this way: and, though neither the right aſcenſions nor the declinations are laid down by it, nor the diſtances between the ſtars meaſured; yet their *apparent* ſituations being preſerved in black and white, with the day and year, and hour if thought neceſſary, written underneath, each card becomes a regiſter of the then appearance of that ſmall portion of the heavens; which is eaſily re-examined at any time with little more than a tranſient view; and which yet will ſhew on the firſt glance, if there ſhould have happened in it any variation of conſequence. It is obvious, that very delicate obſervations are not to be made in this way.

In order to explain my meaning more fully, a card ſo marked ſhall accompany this paper (ſee tab. V. fig. 1.). What I firſt happened to pitch upon was the conſtellation of Corona Borealis, which then fronted one of my windows; and which I have ſince purſued throughout in this method; making the ſtars α, β, γ, δ, ε, ζ, ϑ, ι, $\varkappa$, π, ϱ, σ, and τ, ſucceſſively central; together with one or two belonging to Bootes, for the ſake of connecting the whole together. Theſe I have transferred ſince on a ſheet of paper, to try how well they would unite into one map; which they have done with very little alteration. A copy of that ſhall alſo be laid before this Society (fig. 2.).

My deſign was, after marking down all ſuch ſtars as are viſible with ſo ſmall a magnifier, to go over the whole again with another teleſcope of a higher power, divided in the ſame way; and after that, with a third and a fourth; ſo as to comprehend every ſtar I could diſcern. That would diſcover ſmaller changes: but it muſt be a work of time, if attempted at all. After ſuch a rough map of the conſtellation is made, the endeavouring to aſcertain the right aſcenſions and declinations of theſe, may perhaps be adviſeable in the next place, rather than ſearching for more.

In obſerving in this way it is manifeſt, that the places of ſuch ſtars as happen to be under or very near any one of the wires, muſt be more to be depended upon, than of what are in the intermediate ſpaces, eſpecially if towards the edges of the field: ſo alſo what are neareſt to the centre, becauſe better defined, and more within the reach of one wire or another. For this reaſon, different ſtars in the ſame ſet muſt ſucceſſively be made central, or brought towards one of the wires, where any ſuſpicion ariſes of a miſtake, in order to approach nearer to a certainty: but if the ſtand of the teleſcope be tolerably well adjuſted and fixed in its place, that is ſoon done.

In ſuch a glaſs it is very ſeldom that light is wanting ſufficient to diſcern the wires. When an illuminator is required, I find, that for this purpoſe, where you wiſh to ſee every ſmall ſtar you can, a piece of card or white paſte-board, projecting on one ſide beyond the tube, and which may be brought forward occaſionally, is better than one of any other kind. By cutting acroſs a ſmall ſegment of the object-glaſs, it throws a ſufficient light down the tube, though a candle is at a great diſtance; and one may loſe ſight of that falſe glare when one pleaſes, by drawing back the head, and moving the eye a little ſide-ways, and then one ſees the ſmaller ſtars juſt as well as if no illuminator were there.

This then is the method I would recommend to the practical aſtronomer, for becoming acquainted with the appearance of the ſtars, and ſetting a watch over the heavenly motions. After a very few trials, every one would find this eaſy. And if each perſon of every rank among aſtronomers would take a conſtellation or two under his care, the numbers who could undertake it in this way would compenſate for the defects of a plan which cannot aſpire at great accuracy. The labour of it, even at firſt, is but little. It has coſt me more time indeed than I ought commonly to allot to mere amuſement; becauſe I had my apparatus to contrive, and ſeveral different and fruitleſs ſchemes to try, before I could ſatisfy myſelf. But a quarter, or at the moſt half, an hour is generally ſufficient for the marking of one pretty full card in this way: and when once the cards are marked, and a general map of the conſtellation is formed, a little time given to it in a fine evening, to examine whether the ſtars on ſuch or ſuch a card remain in their former poſition, is little trouble indeed. Perſeverance is moſt likely to be wanting, and therefore muſt be determined upon; becauſe, after finding things time after time juſt as they were, one's hopes of diſcovering any thing new will ſlacken. But the different ſtate of the air, or of one's own eye, will frequently occaſion a freſh ſtar to become viſible, or a ſmall one which had been noted down to ſeem to have diſappeared; and ſuch a mere accident will ſerve to re-kindle the deſire of purſuing it. Beſides, if we obſerve no change after a tolerable interval of aſſiduous ſearch, we may at any time turn to another conſtellation: yet ought we never to abandon the former entirely, after having once publickly undertaken it, without giving notice of our ſo doing.

In the cards or maps, it may be obſerved, I have not marked the reſpective ſizes of the ſtars. Nor have I diſtinguiſhed them in any way, excepting a few of them with Bayer's Greek letters. It was becauſe I have not hitherto ſatisfied myſelf how to do it. Some method muſt be uſed by every one, to deſcribe to himſelf what he means; but, in laying any thing before the public, a deference ought to be paid to what has been done by others. The calling any ſtar by a new name would breed confuſion: and as I was deſirous this ſhould appear before this

Society in its firſt rude form, that a judgement might be made from it how far ſuch a ſcheme would promiſe ſucceſs, I was unwilling to look into catalogues or capital maps for the numbers or names of the ſtars, leſt I ſhould be tempted to adapt the poſitions of what I had obſerved to what I there found ſet down by more able aſtronomers. Nothing, therefore, but a hemiſphere of SENEX has been conſulted, juſt for knowing how far the conſtellation is uſually reckoned to extend, and what are BAYER's references.

Should this plan meet with approbation, I ſhall be happy to have propoſed it; and will endeavour to forward it in any way that ſhall be judged proper: or ſhould any other be preferred, which is within the abilities and leiſure of one who is engaged in another profeſſion, I ſhall be as happy to lend what aſſiſtance I can to it. My aim is only, to render ſuch obſervations as I am capable of making, uſeful to ſcience.

Before I conclude on this head, give me leave to add a few hints. Whether this method be followed, or any other, if a *general plan* be ſet on foot, whoever undertakes a conſtellation, or diſtrict, ſhould determine to examine it with as great accuracy as he can; yet never be aſhamed to let others know of his miſtakes. The error of one proves a caution to another. Such a rough ſketch, once made, will be found of great uſe to moſt of us, in knowing which ſtar next to examine with greater care. He who can do no more than this, will do a uſeful work by going thus far: and his frequently ſweeping over his diſtrict in this way, may lead him to a diſcovery which might eſcape a more regular aſtronomer. But whoever can, ought to do more. By degrees the exact poſitions of every ſtar he has noted down may be aſcertained, by the method practiſed by Mr. DE LA CAILLE in his Southern Hemiſphere, or by any other which ſhall be eſteemed more convenient. Every one, indeed, muſt uſe ſuch inſtruments as he can procure: but aſſiduity can do more with indifferent ones, than will ever be accompliſhed with the very beſt without it. Whatever references are made for one's own convenience, when a map and catalogue are given to the public ſtock, the old letters and numbers ſhould be retained as far as they go: though yet notice ſhould be taken, where the magnitudes of the ſtars at preſent do not appear to correſpond with the order in which they have been laid down.

To render this more complete, it were to be wiſhed, that each ſhould give in a copy of his original obſervations, with an account of the inſtruments he uſed; ſince they ought to be preſerved as data from whence his deductions were made, which may then be re-examined at any future time. Yet muſt it be deſired, that no one would truſt himſelf without carrying on his calculations as faſt as the obſervations are made: they will otherwiſe multiply upon his hands till the labour will dishearten him from attempting it at all. A heap of crude, undigeſted obſervations would be an unwelcome preſent to the public.

Having thus ſtated this Propoſal, I ſhall leave it to be proceeded upon, or not, as ſhall be ſeen proper: And will now only ſubjoin a Liſt of ſuch occaſional obſervations as I have had opportunity of making, ſince the laſt which I communicated to this Society. I find, indeed, that it is much longer than I had apprehended: but as I perceive ſome aſtronomers abroad have referred to a few of thoſe which have been honoured with a place in our Tranſactions, it may be as well to follow it up. An obſervation retained among one's own private papers I hold to be of little uſe.

One thing let me deſire Foreigners to remark: that the regiſters I gave of the going of my clock were meant only as the relations of a *mere fact*; that a clock, of ſuch a conſtruction, kept or altered its rate *ſo* or *ſo*. They ſeem to have underſtood it as an account of a capital clock, by valuing themſelves upon ſome of theirs going better. The time-keepers in moſt of our Obſervatories are far more accurate; but, excepting thoſe of the Royal Obſervatory at Greenwich, their accuracy is not made public.

Another remark it may alſo be proper to make; that, ſince my former papers, the longitude of this place has been aſcertained by comparative obſervations on the burſting of ſome rockets, let off on purpoſe; which, on a mean of ſeveral, turns out to be 19″,02 in time E. of Greenwich Obſervatory; that is, it may hereafter be conſidered as 19″, inſtead of 18″,6 as I had before calculated it trigonometrically from the bearings.

Obſervations made at Chiſlehurſt, *in* Kent, *longitude* 19″ *in time Eaſt of the Royal Obſervatory at* Greenwich, and *latitude* 51° 24′ 33″ *North.*

Eclipſe of the moon, ♂ July 30, 1776: obſerved with a 3½ feet achromatic teleſcope, and a power magnifying 29 times (that is, a ſingle eye-glaſs belonging to the day-tube) the aperture of the teleſcope being reduced to 1½ inches. The night very clear and ſtill.

Apparent time. h.	′	″	
			The beginning not properly obſerved.
10	11	31	Grimaldus touched by the ſhadow.
10	12	49	———— covered.
10	14	5	Galilæus covered.
10	19	36	Ariſtarchus covered.
10	26	0	The ſpot in Kepler biſected.
10	24	25	Schikardus (but Q.) touched.
10	25	52	- - - - - - biſected.
10	27	19	- - - - - - covered.
10	28	15	Copernicus touched.
10	29	49	- - covered.
10	31	22	Helicon (but Q.) covered.
10	37	9	Plato touched.
10	37	54+	- covered.
10	38	55	Tycho touched.
10	39	39	- - biſected.
10	40	25	- - covered.

Manilius

Apparent time.
h. ′ ″

10 43 16 Manilius covered.

10 46 51 Menelaus covered.

10 48 5 Dionyſius covered.

10 55 4 Cenſorinus covered.

10 58 57 A point (Promontorium acutum, I believe) touched.

11 0 21 A ſpot between M. Fœcunditatis and M. Nectaris touched.

11 0 23 M. Criſium touched.

11 3 55 - - - covered.

11 7 57 The eclipſe ſeemingly total.

11 11 11 The moon covers a ſmall ſtar near her ſouth limb. The ſtar hangs on the limb, before it diſappears.

11 28 17 She covers another ſtar a little ſouth of her centre. This vaniſhes inſtantaneouſly.

Theſe occultations were obſerved with another power of the ſame teleſcope; which is uſually reckoned 100, and which I have formerly ſo called; but which on an accurate examination really magnifies almoſt 75 times.

The emerſions of theſe ſtars were not obſerved.

12 43 0 I judge the beginning of the emerſion to be about this time; but cannot be certain.

12 48 1 Grimaldus quitted by the ſhadow.

12 58 25 Ariſtarchus quitted.

12 59 22 Kepler biſected.

13 0 15 Tycho begins to emerge.

13 1 9 - - biſected.

13 1 53 - - emerges. Till this time I had uſed the whole aperture (3,6) having forgotten to reduce

Apparent time. h.	′	″	
			it, till the moon's brightneſs reminded me. Same power as at firſt; that is, 29.
13	6	51	Copernicus begins to emerge.
13	7	20	- - - ſeemingly biſected.
13	8	19	- - - emerges.
13	10	27	Helicon emerges.
13	15	26	Plato begins to emerge.
13	16	31	- - emerges.
13	21	30	Manilius emerges.
13	23	54	Dionyſius emerges.
13	24	57	Menelaus emerges.
13	29	47	Cenſorinus emerges.
13	31	21	The ſpot by M. Fœcunditatis emerges.
13	35	31	The point of Prom. Acutum emerges.
13	37	21	+ M. Criſium begins to emerge.
13	40	26	- - - quitted by the ſhadow.
13	42	0	The end of the eclipſe.
			The air was very clear and ſtill the whole time: the ſhadow but ill defined. Indeed, it was little more than a penumbra; the principal ſpots remaining always viſible on the moon's duſky face.

Eclipſe of the ſun ☿ June 24, 1778: obſerved with a 3½ feet achromatic teleſcope magnifying 75 times. The aperture reduced to two inches, to prevent breaking the ſmoked glaſſes.

h.	′	″	
3	41	33,5	Beginning. I ſuſpect the minute to be miſtaken, and that it ſhould be 3 h. 40′ 33″,5. The firſt

Apparent time.			
h.	′	″	
			impreſſion could not be 2″, I believe not 1″, before I obſerved it.
5	25	24	End. An undulation on the ſun's limb; but the obſervation pretty good.

Eclipſe of the moon ♂ November 23, 1779: obſerved with the ſame teleſcope, magnifying 75 times. The aperture reduced to two inches. Night clear and froſty. No wind.

			The beginning not aſcertained.
6	13	19	Grimaldus touched by the ſhadow.
6	13	28	- - - covered.
6	17	29	Ariſtarchus covered.
6	20	46	Kepler biſected.
6	23	40	M. Humorum touched.
6	27	47	Helicon covered.
6	28	40	Copernicus and Timocharis both biſected.
6	29	57	M. Humorum covered.
6	33	50	Plato touched.
6	34	27	- - covered.
6	41	52	Tycho touched.
6	43	8	- - covered.
6	47	11	Plinius (but *Q.*) covered.
6	59	1	M. Criſium touched.
7	3	16	- - - covered.
7	7	31	The eclipſe total.
8	46	23	Moon's edge begins to emerge.
8	51	14	Grimaldus begins.
8	52	1	- - - emerges.
			A haze comes on.

Apparent time.
h. ′ ″
9 2 23:: Kepler biſected. This not clearly ſeen.
9 11 41 Plato begins to emerge.
9 12 35 - - emerges.
9 13 46 Tycho emerged.
The haze comes on again too much for the obſervation to be purſued any farther.

Eclipſe of the ſun ♂ Oct. 16, 1781: obſerved with the ſame teleſcope and magnifying power.

The beginning not viſible; ſun too low.
20 22 13.5 The end. Good.

Eclipſe of the Moon ☿ Sept. 10, 1783: obſerved with the ſame teleſcope, *viz.* 3½ feet achromatic, with the aperture reduced to two inches; but with a ſmall magnifying power of 36 times, which I had made by Mr. DOLLOND for theſe obſervations, and which I found very convenient. Night a little hazy, but pretty favourable.

9 33 0 A duſkineſs comes on the moon.
9 45 35 The beginning of the ſhadow, I believe.
9 47 20 A hazineſs obſcures the moon.
9 50 55 Ariſtarchus covered.
9 52 20 Kepler covered. So it is ſet down; but I do not recollect what I meant by this; whether it might not be only the ſpot in the centre, ſo that it might more properly be called biſected.

Gaſſendus

Apparent time.
h. ′ ″

9 57 57 Gaſſendus covered. I ſuſpect the minute here; and that it ſhould be 56′ 57″.

9 59 41 Heraclides covered.

10 1 42 Copernicus touched.

10 3 5 - - - covered.

10 3 26 Helicon covered.

10 4 12 Bulialdus covered.

10 8* 0 A hazineſs again.

10 8 57 Plato covered.

10 15 30 Manilius covered.

10 15 54 Tycho touched.

10 17 5 :: - - covered. This doubtful.

10 19 10 Menelaus covered.

10 21 38 Dionyſius covered.

10 22 40 Plinius covered,
A hazineſs again.

10 28 25 Cenſorinus covered.

10 34 34 M. Criſium touched.

10 39 45 - - - covered.

10 46 34 Total darkneſs, as I judged it.

At 10 h. 41′ the moon had grown reddiſh, and the eclipſed part become more viſible than before. After ſome time, during the total darkneſs, the moon was barely to be ſeen. In general, about the centre, it was darker than towards the circumference, which was ill-defined. About

12 0 0 The eaſtern limb became more viſible, and better defined.

The

Apparent time. h.	′	″	
12	14	0	The light ſpreads a great way over the moon from that ſide towards the centre, extending about two-thirds of her circumference (ſee fig. 3.)
12	23	0	The moon ſeems beginning to emerge.
12	25	0	Emerſion certainly has begun.
12	28	21	Grimaldus emerged.
12	31	40	Galileus emerged.
12	33	52	Ariſtarchus emerged.
12	37	26	Kepler (but Q. this as before).
12	39	36	Heraclides emerged.
12	42	56	Helicon emerged.
12	45	52	Copernicus emerged entirely.
12	47	22	Plato begins to emerge.
12	47	58	- - emerges.
12	48	30	Tycho begins to emerge.
12	49	58	- - emerges.
12	58	8	Manilius emerges.
13	1	40	Menelaus emerges.
13	3	18	Dionyſius emerges.
13	5	40	Plinius emerges.
13	11	22	Cenſorinus (but Q.) emerges.
13	16	35	M. Criſium begins to emerge.
13	20	53	- - - emerges.
13	25	38	The ſhadow quits the moon near Langrenus, between that and M. Criſium. The duſkineſs does not leave the moon till ſome time afterwards, but I did not wait to obſerve it.

The moon was darker during the eclipſe than uſual; but the air was not clear enough for any occultations of ſtars to be obſerved.

Tranſit of Mercury over the ſun's diſk ♂ Nov. 12, 1782: obſerved with the ſame teleſcope, and a power of 75 times. The aperture reduced to two inches.

Apparent time.
h. ′ ″

2 51 49 Firſt impreſſion obſerved. It could not be 2″ ſooner.

2 54 57 Thread of light completed; but ſeen through clouds. The planet ſeemed to hang on the ſun's limb 30″ at leaſt.

4 6 0 Through a break in the clouds, of ſhort duration, ☿ ſeemed to have quitted the ſun; but indeed the clouds were very unfavourable the whole time.

Occultation of Saturn by the moon, ♄ February 18, 1775: obſerved with the ſame teleſcope; and, I believe, the ſame power, with the whole aperture of the object-glaſs 3,6 inches; but, I perceive, I have not ſet down theſe particulars.

9 5 39 Præc. anſa of the ring im.

9 6 9 Præc. limb of the planet im.
Subſequent limb not ſet down.

9 6 48 Subſequent anſa im.
The moon low at theſe immerſions, and much undulation. The emerſions loſt by looking at a wrong part of the moon's diſk, except

10 1 7 Subſequent anſa emerges.
Night very clear; but the obſervation on the whole imperfect.

Occultations of ſtars by the moon: obſerved with the ſame teleſcope, and a power of 75 times, with the whole aperture of the object-glaſs.

		Apparent time.			
		h.	'	"	
1775.					
♂ Aug. 1.	☽ γ Virginis	7	48	17	Both ſtars viſible when a cloud covered them.
		7	49	20	A ſhort break; only one ſtar viſible.
		7	52	15	Another break; but before this the ſecond ſtar was immerged.
		8	48	58,5	Firſt ✱ em. good.
		8	49	6,5	Second ✱ em. good.
	☽ a bright ✱ N of γ Virginis	8	54	13	Im. good.
					Em. not till the moon was too low.
♂ Dec. 12.	☽ Regulus	10	5	46	Em. very good, though the moon low.
1776.					
☉ June 30.	☉ 1 ad μ ♐	9	3	49	Im good; ſome flying clouds.
		10	6	38	Em.; perhaps ſooner.
1777.					
♄ Aug. 23.	☽ μ Ceti	10	41	17	Im.: the moon low; night clear and ſtill.
		11	32	10	Em.
♄ Nov. 15.	☽ 1 ad δ Tauri				Im. not ſeen; undulation too great.
		7	22	56	Em. pretty good.
☉ Nov. 16.	☽ ζ Tauri	11	17	1,5	Im. good. Theſe were obſerved with a power of 67 times, and an oblique ſpeculum.
		12	23	28	Em. good.
1783.					
♀ May 16.	☽ π Scorpii	11	21	49	Im. Night clear and ſtill; the obſervations good.
		12	31	49,5	Em.
♃ Jul 10.	☽ π Scorpii				Im. not ſeen for clouds.
		8	43	56	Em.; it might be 1" or 2" ſooner; the moon's edge ill defined.
♂ Dec. 30.	☽ δ Piſcium	8	3	13	Im dark limb, very good.
		9	8	30	Em. good. It could not be above 1" ſooner, if that. Night very clear and ſtill; hard froſt; therm. 13°½.

Eclipſes of Jupiter's ſatellites: obſerved with the ſame teleſcope and power (that is, 75 times; called uſually 100) and whole aperture.

		Apparent time. h.	′	″	
1775.					
♀ Sept. 8.	1 Sat.	11	33	14	Im. flying clouds; obſervation doubtful.
☉ Oct. 1.	1 Sat.	11	51	1	Im. good; unleſs the minute be miſtaken.
♃ Nov. 2.	1 Sat.	8	28	2	Im. good.
♃ 16.	2 Sat.	9	0	13	Im. pretty good; air clear, but a cold in my eyes rendered the obſervation not ſatisfactory.
☽ Dec. 18.	1 Sat.	10	45	48	Em. good.
	2 Sat.	11	2	0	Em. pretty good.
☿ 27.	1 Sat.	7	3	48	Em. good.
1776.					
☉ Nov. 17.	3 Sat.	9	38	48,5	Im.; a ſcintillation for ſome ſeconds before it quite diſappeared.
1778.					
♃ May 21.	1 Sat.	9	9	38	Em. good.
	2 Sat.	10	10	±	Em. ſo near the firſt ſatellite; as ſcarcely to be diſtinguiſhable from it for ſome minutes.
♃ June 11.	4 Sat.	9	52	4	Im. good for the fourth ſatellite, yet viſible by fits for ſome ſeconds longer.
♄ 13.	1 Sat.	9	19	6	Em. pretty good.
1779.					
♂ Mar. 9.	1 Sat.	6	59	19	Im.; that is, this was the laſt of my ſeeing it; but, though the night was clear, the ſatellite was too near Jupiter for the obſervation to be ſatisfactory.
♄ May 22.	2 Sat.	11	5	54	Em. good.
1781.					
♃ May 24.	1 Sat.	10	3	31	Em. very good.
♃ 31.	1 Sat.	11	57	35	Em. pretty good.
♄ June 16.	1 Sat.	10	13	13	Em.; clouds, but pretty good.
1782.					
♄ July 20.	3 Sat.	9	6	42	Em. good.
	2 Sat.	11	30	30	Em. good.

			Apparent time. h.	′	″	
☉ July	21.	1 Sat.	9	39	50	Emerſion; windy; but good.
♃ Aug.	29.	1 Sat.	8	20	15,5	Em.
♀	30.	4 Sat.	8	52	19	Em.; ſatellite ſeen then, but not diſtinct for ſome time.
1783.						
♂ July	8.	1 Sat.	12	14	13	Im. pretty good.
♄ Aug.	2.	1 Sat.	9	10	31,5	Em. good.
☽	25.	1 Sat.	9	28	54	Em.
♀ Sept.	26	1 Sat.	6	19	44	Em. pretty good, but twilight ſtrong.
♂	30.	3 Sat.	10	3	24	Im. It was viſible only by fits for the laſt 8″. Jupiter near a tree.
♀ Oct.	3.	1 Sat.	8	18	0	Em. pretty good; but the moon below Jupiter.
☉	26.	1 Sat.	8	39	17	Em. Jupiter low and near a tree; great undulation.

EXPLANATION OF THE FIGURES IN TAB. V.

Fig. 1. α Cor. Bor. ☿ Aug. 6, 1783, per night-glaſs. The * marked Sept. 24. was not obſerved till that night, but has continued ſince, and was only overlooked at firſt.

Fig. 2. A map of 107 ſtars, beſides thoſe marked by BAYER, in the conſtellation of Corona Borealis, or the Northern Crown; together with a part of Bootes: laid down from obſervations made 1783 with a night-glaſs furniſhed with croſs-wires; as their relative poſitions were eſtimated by the eye.

Fig. 3. The moon as ſhe appeared (inverted) ☿ Sept. 10, 1783, about a quarter of an hour before ſhe began to emerge from total darkneſs.

Fig. 1.

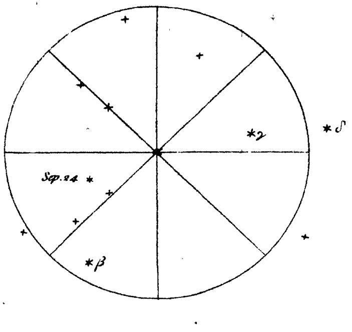


Fig. 2.

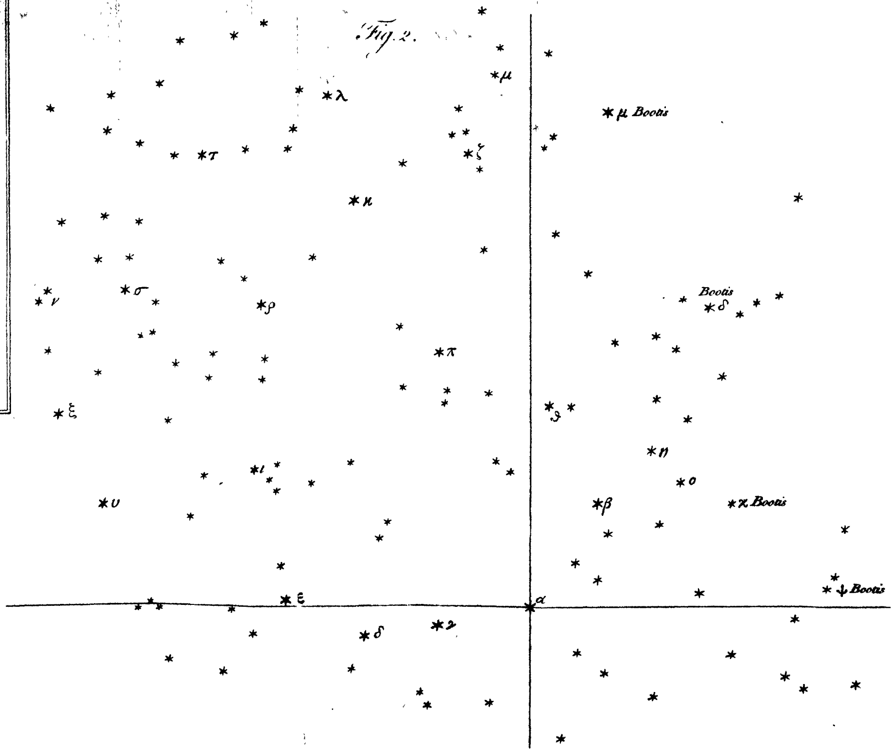
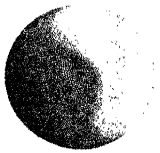


Fig. 3.



Basire sc.

XVIII. *An Account of ſome late fiery Meteors; with Obſervations. In a Letter from* Charles Blagden, *M. D. Phyſician to the Army, Sec. R. S. to Sir* Joſeph Banks, *Bart. P. R. S.*

Read February 19, 1784.

TO SIR JOSEPH BANKS, BART. P. R. S.

DEAR SIR,

FROM the papers you were ſo good as to put into my hands, together with ſuch other information as I could procure, the following account of the two moſt remarkable of the late meteors is collected. I am ſenſible, that it is in many reſpects very imperfect; yet ſtill it gives a more ſatisfactory idea of the phænomena than can well be acquired from the relation of any ſingle obſerver, and therefore may not be diſagreeable to the learned Society over which you ſo worthily preſide, if no more perfect account ſhall previouſly have been laid before them.

Theſe meteors were of the kind known to the ancients by the names of Λαμπαδες, Πιθοι, Bolides, Faces, Globi, &c. from particular differences in their ſhape and appearance, and ſometimes, I believe, under the general term of Comets *; in the

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* ARISTOTLE's remark, that all the comets ſeen among them diſappeared without ſetting, Ἅπαντες δε καθ' ἡμας ωμμενοι (Κομηται) ανευ δυσεως ἀφανισθεντες ἐν τῳ ὑπερ του ὁριζοντος τοπῳ (Meteor. lib. I. c. 6.), ſeems ſcarcely applicable but to tranſitory meteors, and many other expreſſions to the ſame purpoſe occur in that author, PLINY,

Philoſophical Tranſactions they are called indiſcriminately fire-balls or fiery meteors; and names of a ſimilar import have been applied to them in the different languages of Europe.

The moſt material circumſtances obſerved of ſuch meteors may be brought under the following heads. 1. Their general appearance. 2. Their path. 3. Their ſhape or figure. 4. Their light and colours. 5. Their height. 6. Their noiſe. 7. Their ſize. 8. Their duration. 9. Their velocity.

I ſhall begin with the firſt of theſe meteors, that which was ſeen on the 18th of Auguſt.

§ 1. Its general appearance in theſe parts of Great Britain was that of a luminous ball, which roſe in the N.N.W. nearly round, became elliptical and gradually aſſumed a tail as it aſcended, and in a certain part of its courſe ſeemed to undergo a remarkable change compared to burſting; after which it proceeded no longer as an entire maſs, but was apparently divided into a great number or a cluſter of balls, ſome larger than the others, and all carrying a tail or leaving a train behind; under this form it continued its courſe with a nearly equable motion, dropping or caſting off ſparks, and yielding a prodigious light, which illuminated all objects to a ſurpriſing degree; till having paſſed the eaſt, and verging conſiderably to the ſouthward, it gradually deſcended, and at length was loſt out of ſight. The

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PLINY, &c. SENECA himſelf, though he conceived diſtinctly enough the difference between comets and fiery meteors, yet evidently did not know where to draw the line (compare lib. I. and VII. Quæſt. Natur.). Even in modern times, theſe meteors have ſtruck ſpectators at firſt as comets (Fritzes Mediziniſche Annalen, vol. I. p. 73.); nay, expert aſtronomers, as appears by a letter from NATHANIEL PIGOTT, Eſq. F. R. S. lately read before the Royal Society. See alſo Mem. de l'Ac. des Scienc. 1771, p. 688. I have inſiſted the more on a ſubject apparently of ſo little conſequence, in order to account for the ſtrange opinions of the ancients reſpecting comets, which, I think, proceeded chiefly from confounding them with theſe fiery meteors.

time of its appearance was 9 h. 16′ P.M. mean time of the meridian of London, and it continued viſible about half a minute.

§ 2. How far north the meteor may have begun I have no materials to determine with preciſion; but, as it was ſeen in Shetland, and at ſea between the Lewes and Fort William, and appeared to perſons at Aberdeen and Blair in Athol aſcending from the northward, and to an obſerver in Edinburgh as riſing like the planet Mars, there can be little doubt but its courſe commenced beyond the fartheſt extremity of this iſland, ſomewhere over the northern ocean. General MURRAY F. R. S. being then at Athol Houſe, ſaw it paſs over his head as nearly vertical as he could judge, tracing it from about 45° of elevation north-north-weſtward to 30° or 20° ſouth-ſouth-eaſtward, where a range of buildings intercepted it from his view. From near the zenith of Athol Houſe, it paſſed on a little weſtward of Perth, and probably a little eaſtward of Edinburgh; and continuing its progreſs over the ſouth of Scotland, and the weſtern parts of Northumberland and the Biſhopric of Durham, proceeded almoſt through the middle of Yorkſhire, leaving the capital of that county ſomewhat to the eaſtward. Hitherto its path was as nearly S.S.E. as can be aſcertained; but ſomewhere near the borders of Yorkſhire, or in Lincolnſhire, it appears to have gradually deviated to the eaſtward, and in the courſe of that deviation to have ſuffered the remarkable change already noticed under the denomination of burſting. After this diviſion, the compact cluſter of ſmaller meteors ſeems to have moved for ſome time almoſt S.E. thus traverſing Cambridgeſhire and perhaps the weſtern confines of Suffolk; but gradually recovering its original direction, it proceeded over Eſſex and the Straits of Dover, entering the continent probably not

far

far from Dunkirk, where, as well as at Calais and Oſtend, it was thought to be vertical. Afterwards it was ſeen at Bruſſels, Paris, and Nuits in Burgundy *, ſtill holding on its courſe to the ſouthward; nay, I have met with an intimation, though of doubtful authority †, that it was perceived at Rome. Our information of its progreſs over the continent is, indeed, very defective and obſcure; nevertheleſs, I think, we have ſufficient proof that it traverſed in all 13 or 14 degrees of latitude, deſcribing a track of 1000 miles at leaſt over the ſurface of the earth; a length of courſe far exceeding the utmoſt that has been hitherto aſcertained of any ſimilar phænomenon.

To adduce the different accounts from which this path is determined, would not only be inſufferably tedious, but contrary to the intention of this letter, which is to give a ſummary view of the whole. They are contained partly in letters, and partly in the different news-papers of England and Scotland, moſt of which have been peruſed for this purpoſe. The information derived from the news-papers, however incorrect in the detail, is brought to ſome degree of certainty by the check of comparing them with one another; and their frequent publication in moſt places of conſequence in this iſland, procures us advantages on the occaſion of ſuch extraordinary phænomena, not enjoyed in former ages, nor even now, to the like extent, in any other part of the world.

It ſeems ſcarcely more intereſting to trace the path of theſe bodies with minute preciſion, than it would be to mark the progreſs of a cloud's ſhadow upon the ground; but it is of conſequence to their theory to aſcertain well the direction of their courſe; and their deviations from a ſtraight line, as implying ſome particular cauſe, ſhould be carefully noticed. I have

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* Journ. de Paris, Août 24, 1783.

† PARKER's General Advertiſer, October 7, 1783.

have ventured to aſcribe ſuch a deviation to this meteor, from the concurrent teſtimony of many obſervers, who ſpeak in the plaineſt terms of a manifeſt change in its courſe about the time it was ſeen to burſt; and their evidence is confirmed by drawing a line S.S.E. from that part of Scotland to which the meteor was vertical, for ſuch a line is found to correſpond with its path as far as Yorkſhire, but in the ſouthern parts of the kingdom falls a great deal too much to the weſtward. That it afterwards reſumed its former courſe is rendered probable from the teſtimony of the obſervers in Kent, who almoſt uniformly mention its diſappearance in the S.S.E. as well as from the remarks made by ſeveral perſons near the metropolis, that when it attained its greateſt elevation, it bore but one or two points to the northward of eaſt.

§ 3. This meteor was deſcribed by moſt ſpectators under three different forms, and is thus repreſented by Mr. SANDBY in his beautiful Drawing*; but the two firſt of thoſe do not imply any real variation in its ſhape, depending only on a difference in the point of view. Accordingly, in the firſt part of its courſe over Scotland, it was ſeen to have a tail, and is thus deſcribed by General MURRAY when it paſſed Athol Houſe. Two cauſes concur in this deception; firſt, the fore-ſhortening, and even occultation, of the tail, when the object is ſeen nearly in front; and, ſecondly, that the light of moſt part of the tail is of ſo inferior a kind, as to be difficultly perceived at a great diſtance, eſpecially when the eye is dazzled by the overpowering brilliancy of the body. The length and ſhape of the tail, however, were perpetually varying; nor did the body continue always of the ſame magnitude and figure, but was ſometimes round, at other times elliptical, with a blunt or pointed protuberance behind. From ſuch changes of figure in this and other meteors it is, that they have been compared to columns or pyramids of fire, comets, barrels, bottles, flaſks, paper-kites, trumpets, tadpoles, glaſs-drops, quoits, torches, javelins, goats, and many ſimilar objects; whence the multifarious appellations given to them by the ancients were borrowed.

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* Since engraved. See alſo the figures tab. IV. of this volume.

Reſpecting the tails of meteors, it is here neceſſary to diſtinguiſh between two different parts of which they conſiſt. The brighteſt portion ſeems to be of the ſame nature as the body, and indeed an elongation of the matter compoſing it; but the other, and that commonly the largeſt portion, might more properly be called the train, appearing to be a matter left behind after the meteor has paſſed; it is far leſs luminous than the former part, and often only of a dull or duſky red colour. A ſimilar train or ſtreak is not unfrequently left by one of the common falling ſtars, eſpecially of the brighter ſort; and veſtiges of it ſometimes remain for ſeveral minutes. It often happens, that even the large fire-balls have no other tail but this train, and ours of the 18th of Auguſt appeared at times to be in that ſtate; its tail was likewiſe thought by ſome ſpectators to be ſpiral.

Under this changeable form, but ſtill as a ſingle body, it proceeded regularly till a certain period, when expanding with a great increaſe of light, it ſeparated into a cluſter of ſmaller bodies or ovals, each extended into a tail and producing a train. At the ſame time a great number of ſparks appeared to iſſue from it in various directions, but moſtly downward, ſome of which were ſo bright as alſo to leave a ſmall train. Moſt fire-balls have ſuffered a burſting or exploſion of this kind; but in general they have been thought to diſappear immediately afterwards. This, however, continued its courſe, becoming more compact, or perhaps re-uniting, and ſeems to have undergone other ſimilar exploſions before it left our iſland, and again upon the continent*. The different accounts tend to ſhew, that its firſt ſeparation or burſting happened ſomewhere over Lincolnſhire, perhaps near the commencement of the fens. Many obſervers did not get ſight of it till after this period, and therefore never deſcribe it as a ſingle ball. There appears to be ſome deception, in conſequence of which ſpectators are led to believe, that a meteor is extinguiſhed by theſe exploſions; for the ſame opinion was formed of this in ſeveral parts of its courſe, though we have ſuch deciſive evidence of its continued progreſs; whether it be that the meteors really become more dull for a time immediately after their exploſion, or merely appear ſo on account of the greater preceding light, ſince they are always deſcribed as being moſt luminous the inſtant they burſt.

It is obſervable, that the great change in this meteor correſponds with the period in which it ſuffered a deviation from its courſe, as if there was ſome connexion between thoſe two circumſtances; and there are traces of ſomething of the ſame kind having happened to other meteors. If the exploſion be any ſort of effort, we cannot wonder that the body ſhould be moved by it from a ſtraight line; but on the other hand it ſeems equally probable, that if the meteor be forced, by any cauſe, to change its direction, the conſequence ſhould be, a diviſion or ſeparation of its parts.

§ 4. Nothing relative to theſe meteors ſtrikes the beholders with ſo much aſtoniſhment as the exceſſive light they afford, ſufficient to render very minute objects viſible upon the ground in the darkeſt night, and larger ones to the diſtance of many miles from the eye. The illumination is often ſo great as totally to obliterate the ſtars, to make the moon look dull, and even to affect the ſpectators like the ſun itſelf; nay, there are many inſtances in which ſuch meteors have made a ſplendid appearance in full ſun-ſhine. The colour of their light is various and changeable, but generally of a bluiſh caſt, which makes it appear remarkably white. A curious effect of this was obſerved at Bruſſels the 18th of Auguſt, that whilſt the meteor was paſſing, "the moon appeared quite red, but ſoon "recovered its natural light*." The brightneſs alone of the meteor is not ſufficient to explain this, for the moon does not appear red when ſeen by day; but it muſt have depended on the the contraſt of colour, and ſhews how large a proportion of blue rays enters into the compoſition of that light, which could make even the *ſilver* moon appear to have exceſs of red. Priſmatic colours were alſo obſerved in the body, tail, and ſparks of this meteor, variouſly by different perſons; ſome compared them to the hues of gems. The moment of its greateſt brightneſs ſeems to have been when it burſt the firſt time; but it continued long to be more luminous after that period, than it was before.

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* For another inſtance of repeated exploſions conſult Mem. de l'Ac. des Scienc. 1756, p. 23.

The body of the fire-ball, even before it burſt, did not appear of an uniform ſubſtance or brightneſs, but conſiſted of lucid and dull parts, which were perpetually changing their reſpective poſitions; ſo that the whole effect was to ſome eyes like an internal agitation or boiling of the matter, and to others like moving chaſms or apertures. Similar expreſſions have been uſed in the deſcription of former meteors. The luminous ſubſtance was compared to burning brimſtone or ſpirits, Chineſe fire, the ſtars of a rocket, a pellucid ball or bubble of fire, liquid pearl, lightning and electrical fire; few perſons fancied it to be ſolid, eſpecially when it came near the zenith. Different ſpectators obſerved the light of the meteor to ſuffer at times a ſudden diminution and revival, which produced an appearance as of ſucceſſive inflammation; but might, in ſome caſes at leaſt, be owing to the interpoſition of ſmall clouds in its path.

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* From a letter of the Abbé MANN's, Director of the Academy at Bruſſels, to Sir JOSEPH BANKS, Bart. P. R. S.

§ 5. When, in conſequence of a more accurate attention to natural philoſophy, ſuch obſervations were firſt made upon fire-balls as determined their height, the computers were with reaſon ſurpriſed to find them moving in a region ſo far above that of the clouds and other familiar meteors of our atmoſphere; eſpecially as to every uninformed ſpectator they appear extremely near, or as if burſting over his head, a natural effect of their great light when ſeen without intervening objects. Their real height is to be collected from obſervations made at diſtant ſtations, which, for the greateſt accuracy, ought to be ſo ſituated, that the line joining them may cut the path of the meteor at right-angles, and that, at its greateſt elevation, it may appear from both of them about 45° above the horizon, on oppoſite ſides of the zenith. Alſo two ſtations on the ſame ſide of its path, if the leaſt angle of elevation be not very ſmall, and the difference between that and the greateſt angle be conſiderable, are by no means to be rejected. But little reliance can be placed upon obſervations of a meteor's altitude at any ſuppoſed period of its courſe, ſuch as the moment of its burſting; becauſe thoſe changes are ſeldom ſo inſtantaneous,

ſtantaneous, or ſeen ſo much alike by different ſpectators, as to be marked with ſufficient certainty.

Even in proper ſtations it rarely happens, that the angle of elevation can be obſerved with that degree of accuracy, which is neceſſary for any certain determination of the height. An eſtimate by the eye is doubtful, not only on account of the flattened curve the ſky ſeems to deſcribe, for which the moſt experienced obſervers ſcarcely ever make a juſt allowance, but likewiſe of the emotion produced by ſuch an unexpected, magnificent, and perhaps alarming ſpectacle, which renders it almoſt impoſſible to be quite collected. Therefore, unleſs an obſervation be checked by means of a houſe, tree, or ſome fixed body, along which the meteor was found to range, it muſt be received as uncertain. By night the ſtars afford excellent marks, eſpecially if the time be known with exactneſs; the brighter meteors, indeed, render theſe faint lights inviſible for the moment, but here we derive an eminent advantage from the train, which remains after the meteor is gone, and delineates perfectly its track through the heavens. If no ſuch marks have been taken, the expedient of endeavouring to recollect the part of the ſky where it paſſed, and aſcertaining that height with a quadrant, may often be uſeful; but there are many men of ſuch a turn of mind, that the original impreſſion made upon them will be totally perverted by their own ſubſequent reflexions and the remarks of others; in which caſe ſuch an application of inſtruments is likely to give a reſult farther from the truth, than their firſt immediate judgement, however vague and hazarded.

I am ſorry to add, that moſt of the obſervations in my poſſeſſion of the meteor which appeared the 18th of Auguſt, give its altitude by eſtimation only; yet I hope their correſpondence

with

with one another will gain them a degree of credit, to which, if ſingle, they would not be entitled.

1. In a letter from Perth in Scotland, it is ſaid, that "a "gentleman, who has a very good eye, obſerved the meteor "paſs about 6° to the weſtward of the zenith;" and a Profeſſor in one of the Univerſities, being at Ardoch on the banks of the Tweed, about two miles below Dunbarton, judged it to have "at leaſt 45° of elevation above the horizon." Theſe altitudes would make its real height 57 ſtatute miles.

2. At St. Andrew's in Scotland, "it was not quite vertical, "but according to ſome was 20° or 25° from the zenith, ac"cording to others not ſo much." Taking the greateſt of theſe diſtances as neareſt the truth, ſince we are uſually led to eſtimate altitudes greater than they really are, this obſervation, calculated with that of Ardoch, gives 60 miles for the height.

For the communication of theſe obſervations, collected by his friends, I am indebted to General MELVILL F. R. S.

At Edinburgh the meteor paſſed very near the zenith, in which caſe a deviation of a few degrees is ſcarcely perceptible to a common eye.

The rev. Mr. WATSON of Whitby, in a letter to Lord MULGRAVE V. P. R. S. is very confident, that the greateſt altitude of the meteor, which paſſed to the weſtward of his zenith, was 60°. Mr. EDGEWORTH F. R. S. in his letter to you, Sir, ſtates its elevation at Edgeworth's-Town near Mullingar, in Ireland, as 10° or 12° above the eaſtern horizon. Theſe obſervations, calculated ſtrictly from the latitudes and longitudes with the allowance for the curvature of the earth, as indeed were all the reſt where the difference would be ſenſible, give 57 miles for the height of the meteor.

4. In the Morning Chronicle of Sept. 19. is inſerted a letter from Newton Ardes, 7 miles eaſt of Belfaſt, in Ireland, correſponding ſo well with Mr. EDGEWORTH's in the deſcription of the meteor, as to appear very good authority. The altitude is there given as 16°, whence a height of 58 miles with the obſervation at Whitby.

5. Mr. MORE, Secretary to the Society for the encouragement of Arts, Manufactures, and Commerce, ſaw the meteor as he was riding about three miles S.W. of Broſeley in Shropſhire, and judged it to be elevated 35°. By a perpendicular drawn from this ſpot to its ſuppoſed path in Lincolnſhire, its height came out 59 miles.

6. The altitude of 25° determined at Windſor I take to be one of thoſe on which moſt reliance can be placed, becauſe the gentlemen preſent, two of them Fellows of the Royal Society, were remarkably well qualified for ſuch an eſtimation. The letter you received, Sir, from Profeſſor ALLAMAND of Leyden, mentions that the meteor was ſeen there about 30° above the horizon, and the terms in which it is deſcribed in the Dutch news-papers * agree with this account. Its height hence calculated appears to be 58 miles.

7. Mr. THOMAS SQUIRE, of Folkſtone, obſerved the meteor over his houſe, as he was in the poſture of leaning back againſt a hedge; he afterwards tried "its ranging with the roof by a "quadrant, and found it 68°½ above the horizon." Reducing this obſervation to the perpendicular dropped from Windſor on the path of the meteor, its height comes out 54 or 55 miles. Mr. SQUIRE's altitude, determined by a fixed object, is confirmed by the eſtimate of ſeveral perſons at Ramſgate.

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* Amſterdamiſche Courant, Aug. 28, 1783.

8. The meteor was ſeen by Mr. STEEVENS F.,R. S. at Hampſtead near London, moving along over the top of a row of trees. Mr. CAVENDISH F. R. S. having taken the altitude of theſe trees with a quadrant, found that of the higheſt, as ſeen from the part of the garden-walk oppoſite to it, to be 33°; which correſponds very well with the other obſervations, and conſequently gives the ſame height for the meteor. Mr. STEEVENS kept his eye upon it conſtantly, whilſt he paſſed briſkly along the walk.

This agreement of the different altitudes is nearer than could be expected; yet I know of no contradictory obſervations of any authority, except ſome made near Plymouth and in Cornwall, where the meteor being pretty near the horizon, its altitude, as will commonly happen in ſuch caſes, is given too great. The effect of this, however, would be to ſhew, that the meteor was higher; and therefore, I think, we may ſafely conclude, that it muſt have been more than 50 miles above the ſurface of the earth, in a region where the air is at leaſt 30000 times rarer than here below.

Contrary to what has been aſſerted of moſt other fire-balls, this of the 18th of Auguſt appears by the preceding obſervations to have kept on in a parallel courſe, without any deſcent or approach toward the earth. It may be much queſtioned, whether ſuch a deſcent has been proved in any former inſtance. The meteor deſcribed by Sir JOHN PRINGLE has been cited as the moſt certain example; but any perſon who carefully examines the obſervations themſelves, as ſtated in the 51ſt volume of the Philoſophical Tranſactions, will find them totally inadequate for ſuch a concluſion; its height ſeems to me determined only in one part of its courſe, between Iſland-Bridge and Ancram, and was there from 48 to 50 miles [*]. M. LE ROY ſuppoſes the fire-ball ſeen July 17, 1771, to have been 54 miles high when it began, and 27 at its exploſion [†]; but does not give the facts on which his calculation is founded.

Every philoſopher muſt be ſtruck with the agreement of theſe meteors in their diſtance from the earth, juſt beyond the limits of our crepuſcular atmoſphere.

§ 6. That a report was heard ſome time after the meteor of the 18th of Auguſt had diſappeared, is a fact which reſts upon the teſtimony of too many witneſſes to be controverted, and is, beſides, conformable to what has been obſerved in moſt other inſtances. In general it was compared to the falling of ſome heavy body in a room above ſtairs, or to the diſcharge of one or more large cannon at a diſtance. That rattling noiſe, like a volley of ſmall arms, which has been remarked after other meteors, does not ſeem to have been heard on this occaſion. From a compariſon of the different accounts, it appears as if the report was loudeſt in Lincolnſhire and the adjacent countries, and again in the eaſtern parts of Kent; in the intermediate places it was ſo indiſtinct as generally not to have been noticed, and all obſervers of credit in Scotland deny that they heard any thing of the ſort. If, therefore, this report be connected with the burſting of the meteor, I ſhould be inclined to ſuppoſe, that ſound was produced two ſeparate times, namely at the firſt exploſion over Lincolnſhire, and again when it ſeemed to burſt ſoon after entering the continent. Ingenious men have availed themſelves of this ſound, to calculate the diſtance and height of meteors; and the exactneſs attained by this method, in the computation of the late fire-ball from the report heard at

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[*] Phil. Tranſ. vol. LI. p. 241. and 274.

[†] Mem. de l'Acad. des Scienc. 1771, p. 676.

Windſor *, is very remarkable; but in general the accounts diſagreed ſo much, that it would have been impoſſible to conclude any thing from them. Perhaps too the method itſelf is leſs certain than has been thought; for as the propagation of ſound, and with intenſity too, in air rarefied 30000 times, preſents great difficulties in theory, though it may be in ſome meaſure explicable from the vaſt bulk of the meteor, and the large quantity of this rare air it may therefore diſplace by a ſudden expanſion; I think it not improbable, that ſome hitherto unperceived circumſtance comes into play, by which the whole effect may be modified: for inſtance, if matter belonging to the meteor itſelf be what conveys the ſound to our lower atmoſphere, it may either admit ſound to be propagated through it at a different rate than through common air, or it may move much faſter than ſound travels, as the entire meteor certainly does, and carry on the ſonorific vibrations with it. Moreover, we cannot be ſure what is the velocity of ſound in air ſo much rarer than where our experiments have been made. For theſe reaſons, while we diſtruſt calculations of meteors founded on the progreſs of ſound, we ſhould be particularly careful to note down the intervals, and all the circumſtances, as they may lead to very curious diſcoveries. The effect of the noiſe is, frequently, to produce ſuch a ſhaking of the doors, windows, and the whole houſe, as is miſtaken for an earthquake.

Beſides the report as of exploſions which was heard *after* the meteor, another ſort of ſound was ſaid to *attend* it, more doubtful in its nature, and leſs eſtabliſhed by evidence; I mean, a kind of hiſſing, whizzing, or crackling, as it paſſed along. That ſound ſhould be conveyed to us in an inſtant from a body above 50 miles diſtant, appears ſo irreconcilable to all we know of philoſophy, that perhaps we ſhould be juſtified in imputing the whole to an affrighted imagination, or an illuſion produced by the fancied analogy of fireworks. The teſtimony in ſupport of it is, however, ſo conſiderable, on the occaſion of this as well as former meteors, that I cannot venture to reject it, however improbable it may be thought, but would leave it as a point to be cleared up by future obſervers.

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* See p. 111. of this volume.

§ 7. To determine the bulk of the fire-ball, we muſt not only have calculated its diſtance, but alſo know the angle under which it appeared. For this purpoſe the moon is the uſual term of compariſon; but as it was thought, at very different diſtances, to preſent a diſk equal to that luminary's, and the ſame expreſſions have been applied to moſt preceding fire-balls, I conceive this eſtimation rather to be a general effect of the ſtrong impreſſion produced by ſuch ſplendid objects on the mind, than to convey any determinate idea of their ſize. However, if we ſuppoſe its tranſverſe diameter to have ſubtended an angle of 30′ when it paſſed over the zenith, which probably is not very wide of the truth, and that it was 50 miles high, it muſt have been almoſt half a mile acroſs. The tail ſometimes appeared 10 or 12 times longer than the body; but moſt of this was train, and the real elongation behind ſeems ſeldom to have exceeded twice or thrice its tranſverſe diameter, conſequently was between one and two miles long. Now if the cubical contents be conſidered, for it appeared equally round and full in all directions, ſuch an enormous maſs, moving with extreme velocity, affords juſt matter of aſtoniſhment.

§ 8. The duration of the meteor is very differently ſtated, partly becauſe ſome obſervers had it in view a much longer time than others, and partly becauſe they formed different judgements of the time. Thoſe who ſaw leaſt of it ſeem to have perceived its illumination about ten ſeconds, and thoſe who ſaw moſt of it about a minute: hence the various accounts may in ſome meaſure be reconciled. Mr. Herschel F. R. S. at Windſor, muſt have kept it in ſight long after other obſervers had thought it extinct: for though, probably, he did not ſee the beginning, as it never appeared to him like a ſingle ball, he watched it as much as "forty or forty-five "ſeconds, the laſt twenty or twenty-five of which it remained "almoſt in one ſituation, within a few degrees of the horizon." This confirms the foreign accounts of its long progreſs to the ſouthward.

As ſcarcely any one had ſufficient preſence of mind to minute the time by his watch, the periods given for its duration are moſtly by gueſs. To correct this rude conjecture, it has been propoſed, that the obſerver ſhould endeavour to paſs over the time in his own mind as well as he can by recollection, whilſt another perſon ſilently marks the ſeconds with a watch. This may do ſomething, but ſtill leaves the matter very uncertain, as the nature of the emotion felt by the ſpectator while it was paſſing will cauſe the impreſſion of a longer or ſhorter time to be left upon his mind; and the formal proceſs of recollection is ſo tedious, that I believe the duration will in this way generally be made too ſhort. Mr. Herschel, at my requeſt, was ſo good as to act over his obſervation, with the poſitions and geſtures he was obliged to employ; and this ſeems likely to come nearer the truth than a ſimple effort of the mind at recollection. But the ſureſt method would be, to repeat any uniform action in which the ſpectator might have been engaged at the time; as, for inſtance, to walk over the ſame ſpace of ground that he paſſed while the meteor was in ſight.

§ 9. From the apparent motion of the meteor, compared with its height, ſome computation may be formed of its aſtoniſhing velocity. As at the height of 50 miles above the ſurface of the earth, it might be viſible from the ſame ſtation for a tract of more than 1200 miles, and the longeſt continuance of its illumination ſcarcely exceeded a minute, we have hence ſome preſumption that it moved not leſs than 20 miles in a ſecond. The rev. Mr. WATSON, in his letter to Lord MULGRAVE, ſays, *that the arc deſcribed by it whilſt in his view could not be leſs than* 70° *or* 80°, *and yet the time could not exceed* 4″ *or* 5″ *at moſt*. This, with an altitude of 60°, and height of 50 miles, gives for its velocity about 21 miles in a ſecond. The obſerver at Newton Ardes eſtimated its motion to be 10° *in a ſecond, at the altitude of* 16°; this would make its velocity 30 miles in a ſecond. Mr. HERSCHEL found it deſcribe *an arch of* 167° *during the* 40 *or* 45 *ſeconds he obſerved it*, which gives a velocity of more than 20 miles in a ſecond. Finally, Mr. AUBERT F. R. S. thought it deſcribed *an arch of* 136° *of azimuth in* 10 *or* 12 *ſeconds*, which would make its velocity above 40 miles in a ſecond. I am ſenſible of the objections that may be made to all theſe computations; undoubtedly they are too vague; and yet, all taken together, perhaps they may have ſome weight, eſpecially as they correſpond ſo well with the different phænomena of the meteor's duration, and other fire-balls have been computed to move as faſt *. Stating the velocity at the loweſt computation of 20 miles a ſecond, it exceeds that of ſound above 90 times, and begins to approach toward that of the earth in her annual orbit. At ſuch a rate, it muſt have paſſed over the whole iſland of Great Britain in leſs than half a minute, and might have reached Rome within a minute afterwards, or in ſeven minutes have traverſed the whole diameter of the earth!

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* See Mem. de l'Acad. des Scienc. 1771, p. 678. Phil. Tranſ. N° 341. and 360. and vol. LI. p. 263, &c.

From this calculation it will be evident, that there is little chance of determining the velocity of meteors from the times of their paſſing the zenith of different places; and that therefore we muſt principally depend on obſerving carefully, with a watch that ſhews ſeconds, their apparent velocity through the heavens.

THE fire-ball which appeared on the 4th of October, at 43′ paſt ſix in the evening, was much ſmaller than that already deſcribed, and of much ſhorter duration. It was firſt perceived to the northward as a ſtream of fire, like the common ſhooting ſtars, but large; and having proceeded ſome way under this form, it ſuddenly burſt out into that intenſely bright bluiſh light which is peculiar to ſuch meteors. At this period I ſaw it, and can compare the colour to nothing I am acquainted with ſo well, as to the blue lights of India, and ſome of the largeſt electrical ſparks. The illumination was very great; and on that part of its courſe where it had been ſo bright, a duſky red ſtreak or train was left, which remained viſible perhaps a minute even with a candle in the room, and was thought by ſome gradually to change its form. Except this train, I think the meteor had no tail, but was nearly a round body, or perhaps a little elliptical. After moving not leſs than 10° in this bright ſtate, it became ſuddenly extinct, without any appearance of burſting or exploſion.

This meteor was ſeen for ſo ſhort a way, that it is ſcarcely poſſible to determine the direction of its courſe with accuracy; but as in proceeding to the eaſtward it very perceptibly inclined towards the horizon, it certainly moved ſomewhere from the north-weſtward to the ſouth-eaſtward. Its duration was ſo ſhort, that many perſons thought it paſſed in an oppoſite direction; for my own part, I found myſelf abſolutely unable to determine whether the motion was *from* or *toward* the S.E. Some ſpectators were of opinion, that it changed its courſe the moment it became bright, proceeding no longer in the ſame ſtraight line; but my information is not ſufficient to determine this queſtion.

My ſituation, Sir, was particularly fortunate for aſcertaining the height of this meteor, as I ſaw it from your Library, ranging immediately over the oppoſite roof of your houſe. Hence I find by a quadrant that its altitude, even when it became extinct, could not be leſs than 32°. The upper northernmoſt end of the train it left bore, as I judge by the compaſs, about 28° northward of true E. and the lower end about 14°. I have only one obſervation to compare with this, which was made by Mr. BOYS of Sandwich. He concludes, from the train I imagine, that "it diſappeared juſt under, and a very "little to the weſtward" (rather northward) "of, the ſtar "γ in the foot of Cepheus." At that time γ Cephei was about 57° high, and bore above 21° to the eaſtward of N. whence the height of the meteor above the ſurface of the earth, after all proper allowances are made*, muſt have been between 40 and 50 miles.

As there was no appearance of burſting at the extinction of this fire-ball, ſo no report was heard after it; nor did any ſound attend it.

Some obſervers thought *this* meteor alſo near as big as the moon, but to me it did not appear above one quarter of her diameter, which would make its breadth ſomewhat above a furlong.

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* It appears from obſervations taken by Gen. ROY, F. R. S. that the bearing of Sandwich from London is not ſo much to the ſouthward of eaſt, as it is laid down in our maps.

If the whole of the meteor's track be included, it ſeems to have laſted as much as three ſeconds, but in the bright ſtate its duration was leſs than two, I think not much above one. Suppoſing it deſcribed an arc of 14° in 1¼ ſecond, or, according to Mr. AUBERT's obſervation, of 25° in 3″, its real velocity was about 12 miles a ſecond.

Such meteors as theſe, which paſs like a flaſh of lightning, and deſcribe ſo ſhort a courſe, are very unfavourable for calculating the velocity, but afford great advantages for determining the height, as they muſt be ſeen nearly at the ſame moment and in the ſame place by the different obſervers. Other inſtances are found of fire-balls beginning with a dull red light like a falling ſtar, particularly the great one of March 19, 1719, treated of ſo fully by Dr. HALLEY * and Mr. WHISTON †.

It is remarkable, that a ſimilar meteor had appeared the ſame day, that is, Saturday the 4th of October, about three in the morning, though, on account of the early hour, it was ſeen by fewer ſpectators. They repreſent it as riſing from the northward to a ſmall altitude, and then becoming ſtationary with a vibratory motion, and an illumination like day-light; it vaniſhed in a few moments, leaving a train behind. This ſort of tremulous appearance has been noticed in other meteors, as well as their continuing ſtationary for ſome time, either before they began to ſhoot forward, or after their courſe was ended.

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* Phil. Tranſ. vol. XXX. N° 350. p. 978.

† Account of a ſurpriſing meteor ſeen March 19, 1719.

I FIND it, Sir, impoſſible to quit this ſubject, without ſome reflexions about the cauſe, that can be capable of producing ſuch appearances at an elevation above the earth, where, if the atmoſphere cannot abſolutely be ſaid to have ceaſed, it is certainly to be conſidered as next to nothing. The firſt idea which ſuggeſted itſelf, that they were burning bodies projected with ſuch a velocity, was quickly abandoned, from the want of any known power to raiſe them up to that great height, or, if there, to give them the required impetus; and the ingenuity of Dr. HALLEY ſoon furniſhed him with another hypotheſis, in which he thought both theſe difficulties obviated. He ſuppoſes there is no projection of a ſingle body in the caſe; but that a train of combuſtible vapours, accumulated in thoſe lofty regions, is ſuddenly ſet on fire, whence all the phænomena are produced by the ſucceſſive inflammation *. But Dr. HALLEY gives no juſt explanation of the nature of theſe vapours, nor of the manner in which they can be raiſed up through air ſo extremely rare; nor, ſuppoſing them ſo raiſed, does he account for their regular arrangement in a ſtraight and equable line of ſuch prodigious extent, or for their continuing to burn in ſuch highly rarefied air. Indeed, it is very difficult to conceive, how vapours could be prevented, in thoſe regions where there is in a manner no preſſure, from ſpreading out on all ſides in conſequence of their natural elaſticity, and inſtantly loſing that degree of denſity which ſeems neceſſary for inflammation. Beſides, it is to be expected, that ſuch trains would ſometimes take fire in the middle, and ſo preſent the phænomenon of two meteors at the ſame time, receding from one another in a direct line.

Theſe difficulties have induced other philoſophers to relinquiſh Dr. HALLEY's hypotheſis, and propoſe, inſtead of it, one

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* Phil. Tranſ. vol. XXX. N° 360.

one of a very oppoſite nature, that meteors are permanent ſolid bodies, not raiſed up from the earth, but revolving round it in very eccentric orbits; or, in other words, that they are terreſtrial comets *. The objections to this opinion, however, ſeem to me equally great. Moſt obſervers deſcribe the meteors, not as looking like ſolid bodies, but rather like a fine luminous matter, perpetually changing its ſhape and appearance. Of this many defenders of the opinion are ſo ſenſible, that they ſuppoſe the revolving body gets a coat or atmoſphere of electricity, by means of which it becomes luminous; but, I think, whoever carefully peruſes the various accounts of fireballs, and eſpecially ours of the 18th of Auguſt when it divided, will perceive that their phænomena do not correſpond with the idea of a ſolid nucleus enveloped in a ſubtile fluid, any more than with the conjecture of another learned gentleman, that they become luminous by means of a contained fluid, which occaſionally explodes through the thick ſolid outer ſhell †.

A ſtrong objection to this hypotheſis of permanent revolving bodies, is derived from the great number of them there muſt be to anſwer all the appearances. Such a regular gradation is obſerved, from thoſe large meteors which ſtrike all beholders with aſtoniſhment, and occur but rarely, down to the minute fires called ſhooting ſtars, which are ſeen without being regarded in great numbers every clear night, that it ſeems impoſſible to draw any line of diſtinction between them, or deny that they are all of the ſame nature. But ſuch a crowd of revolving bodies could ſcarcely fail to announce their exiſtence by ſome other means than merely a luminous train in the night;

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* See a diſſertation on this ſubject by Profeſſor CLAP, of Yale College, New England.

† Phil. Tranſ. vol. LI. p. 267.

as, for inſtance, by meeting or juſtling ſometimes near the earth, or by falling to the earth in conſequence of various accidents; at leaſt we might expect they would be ſeen in the day-time, either with the naked eye of teleſcopes, by ſome of the numerous obſervers who are conſtantly examining the heavens. With regard to theſe falling ſtars, it were much to be wiſhed, that obſervations ſhould be made upon them by different perſons in concert at diſtant ſtations, for the purpoſe of aſcertaining their height and velocity; which would tend very much to illuſtrate all this part of meteorology.

Another argument of great weight againſt the hypotheſis that fire-balls are terreſtrial comets, is taken from their great velocity. A body falling from infinite ſpace toward the earth, would have acquired a velocity of no more than 7 miles a ſecond, when it came within 50 miles of the earth's ſurface; whereas theſe meteors ſeem to move at leaſt three times faſter. And this objection, if there be no miſtake in regard to the velocity of the meteors, as I think there is not, abſolutely overſets the whole hypotheſis.

What then can theſe meteors be? The only agent in nature with which we are acquainted, that ſeems capable of producing ſuch phænomena, is electricity. I do not mean that by what is already known of that fluid, all the difficulties relative to meteors can be ſolved, as the laws, by which its motions on a large ſcale are regulated in thoſe regions ſo nearly empty of air, can ſcarcely, I imagine, be inveſtigated in our ſmall experiments with exhauſted veſſels *; but only that ſeveral of the facts point out a near connexion and analogy with electricity, and that none of them are irreconcilable to the diſcovered laws of that fluid.

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* How nearly the phænomena of meteors have been repreſented by artificial electricity is known from a very remarkable experiment of Mr. ARDEN's. See PRIESTLEY, vol. V. p. 379.

1. Electricity moves with ſuch a prodigious velocity, as to elude all the attempts hitherto made by philoſophers to detect it; but the ſwiftneſs of meteors, ſtating it at 20 miles a ſecond, is ſuch as no experiments yet contrived could have diſcovered, and which ſeems to belong to electricity alone. This is, perhaps, the only caſe in which the courſe or direction of that fluid is rendered perceptible to our ſenſes, in conſequence of the large ſcale on which theſe fire-balls move.

2. Various electrical phænomena have been ſeen attending meteors. Lambent flames are deſcribed as ſettling upon men, horſes, and other objects*; and ſparks coming from them, or the whole meteor itſelf, it is ſaid, have damaged ſhips, houſes, &c. in the manner of lightning †. Theſe facts, I muſt own, are but obſcurely related, yet ſtill they do not ſeem to be deſtitute of foundation. If there be really any hiſſing noiſe heard while meteors are paſſing, it ſeems explicable on no other ſuppoſition than that of ſtreams of electric matter iſſuing from them, and reaching the earth with a velocity equal to that of the meteor, namely, in two or three ſeconds. Accordingly, in one of our late meteors, the hiſſing was compared to that of electricity iſſuing from a conductor ‡. The ſparks flying off ſo

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* PRIESTLEY's Hiſtory of Electricity, p. 352. Mem. de l'Acad. des Scienc. 1771, p. 681, 682. See an odd fact, perhaps of this nature, in PARKER's General Advertiſer, Dec. 1, 1783.

† Mem. anc. de l'Acad. de Dijon, tom. I. Hiſt. p. 42. Phil. Tranſ. vol. XLVI. p. 366. Hiſt. de l'Acad. des Scienc. 1761, p. 28.

‡ Cheſter Weekly Courant, Auguſt 26, 1783. This and many other curious circumſtances, relative to meteors, are ſo well exemplified in the following obſervation, made ſeveral years ago by Mr. ROBINSON at Hinckley in Leiceſterſhire, that I think it worth tranſcribing here, eſpecially as it occurs in a work which few people would think of conſulting on ſuch a ſubject. "Oct. 26, "1766, at half paſt five in the evening, after a violent ſtorm of wind and rain,

perpetually from the body of fire-balls, may poſſibly have ſome connexion with theſe ſtreams *. In the ſame manner the ſound of exploſions may perhaps be brought to us quicker, than if it were propagated through the whole diſtance by air alone. Should theſe ideas be well founded, the change of direction which meteors ſeem at times to undergo, may poſſibly be influenced by the ſtate of the ſurface of the earth over which

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“ I obſerved a fiery meteor. Its direction was from N.W. to S.E. nearly in a horizontal direction; it paſſed very near to me, and was of an elliptical form; its motion about 40° in 2″ or 3″ of time. It was very bright and lucid to appearance like the paleſt lightning, and emitted ſparks continually, which formed a kind of tail toward the N.W. which ſeemed to be extinguiſhed at the diſtance of 2° or 3° from the body; there was a ſmall portion that parted from it. The coheſion of matter was ſo great, that it drew a thread of conſiderable length from the body, before it broke from it. During the paſſage there was a kind of *hiſſing noiſe, much like to what we hear from the electrical machine when the electric matter is running away, or as when it is eſcaping from a full charged jar.*” Bibliotheca Topographica Britannica, N° VII. p. 81.

* Hiſt. de l'Acad. des Scienc. 1761, p. 28. Mem. de l'Acad. des Scienc. 1771, p. 682. Extract of a letter from the Abbé MANN, Director of the Academy at Bruſſels, to Sir JOSEPH BANKS, Bart. P. R. S. “ I ſhall only mention one ſingular circumſtance, which was communicated to me by a particular friend of mine. It happened at Mariekercke, a ſmall village on the coaſt, about half a league to the W. of Oſtend. The curate of the village was ſitting in the duſk of the evening with a friend, when a ſudden light ſurpriſed them, and immediately after a ſmall ball of light-coloured flame came through a broken pane of glaſs, croſſed the room where they were ſitting, and fixed itſelf on the chink of a door oppoſite to the window where it entered, and there died gradually away. It appeared to be a kind of phoſphoric light, carried along by the current of air. The curate and his friend, greatly ſurpriſed at what they ſaw, apprehended fire in the neighbourhood; but going out, found that the fire, which had come in through the window, had been detached from a large meteor in its paſſage.”

How far theſe and ſimilar appearances may be owing ſimply to the illumination produced by meteors, ſhould be attentively conſidered in the inveſtigation of ſuch facts.

they are paſſing, and to which the ſtreams are ſuppoſed to reach. A ſimilar cauſe may occaſion the apparent exploſion, the opening of more channels giving new vent and motion to the electric fluid. May not the deviation and exploſion which appear to have taken place in the fire-ball of the 18th of Auguſt over Lincolnſhire, have been determined by its approach toward the fens, and an attraction produced by that large body of moiſture?

3. A further argument for the electric origin of meteors is deduced from their connexion with the northern lights, and the reſemblance they bear to theſe electrical phænomena, as they are now almoſt univerſally allowed to be, in ſeveral particulars. Inſtances are recorded, where northern lights have been ſeen to join and form luminous balls, darting about with great velocity, and even leaving a train behind like the common fire-balls *. This train I take to be nothing but the rare air left in ſuch a highly electrified ſtate as to be luminous; and ſome ſtreams of the northern lights are very much like it. The *aurora borealis* appears to occupy as high, if not a higher, region above the ſurface of the earth, as may be judged from the very diſtant countries to which is has been viſible at the ſame time †; indeed the great accumulation of electric matter ſeems to lie beyond the verge of our atmoſphere, as eſtimated by the ceſſation of twilight. Alſo with the northern

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* Hiſt. de l'Acad. des Scienc. 1705, p. 35. WHISTON's Account of a Meteor ſeen in the Air 171$\frac{1}{8}$. Phil. Tranſ. vol. XLI. p. 626; and LIII. p. 6? Alſo a moſt pointed fact in the Act. Liter. Succiæ, 1734, p. 78.

† BERGMAN, upon a mean of 30 computations, makes the average height of the northern lights to be near 70 Swediſh, that is, about 460 Engliſh miles. Kong. Vetenſk. Acad. Handlingar, vol. XXV. p. 193. See alſo Phil. Tranſ. vol. LIV. p. 327. and M. DE MAIRAN's Traité de l'Aurore Boreale, p. 51.

lights a hiſſing noiſe is ſaid to be heard in ſome very cold climates; GMELIN ſpeaks of it in the moſt pointed terms, as frequent and very loud in the north-eaſtern parts of Siberia*; and other travellers have related ſimilar facts †.

But,

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[*] Reiſe durch Siberien, vol. III. p. 135. As the whole paſſage is very remarkable, and has never, that I know, appeared in Engliſh, I thought the following tranſlation of it might be acceptable.

" Theſe northern lights begin with ſingle bright pillars, riſing in the N. and " almoſt at the ſame time in the N.E. which gradually increaſing comprehend a " large ſpace of the heavens, ruſh about from place to place with incredible velo-" city, and finally almoſt cover the whole ſky up to the zenith. The ſtreams are " then ſeen meeting together in the zenith, and produce an appearance as if a " vaſt tent was expanded in the heavens, glittering with gold, rubies, and ſap-" phire. A more beautiful ſpectacle cannot be painted; but whoever ſhould ſee " ſuch a northern light for the firſt time, could not behold it without terror. " For however fine the illumination may be, it is attended, as I have learned from " the relation of many perſons, with ſuch a hiſſing, cracking, and ruſhing noiſe " throughout the air, as if the largeſt fire-works were playing off. To deſcribe " what they then hear, they make uſe of the expreſſion, *Spolochi chodjat*, that is, " the raging hoſt is paſſing. The hunters who purſue the white and blue foxes " in the confines of the Icy Sea, are often overtaken in their courſe by theſe " northern lights. Their dogs are then ſo much frightened, that they will not " move, but lie obſtinately on the ground till the noiſe has paſſed. Commonly " clear and calm weather follows this kind of northern lights. I have heard this " account, not from one perſon only, but confirmed by the uniform teſtimony " of many, who have ſpent part of ſeveral years in theſe very northern regions, " and inhabited different countries from the Yeniſei to the Lena; ſo that no " doubt of its truth can remain. This ſeems indeed to be the real birth-place " of the *aurora borealis*."

It is here to be obſerved, that GMELIN did not collect the account himſelf, but extracted it from letters or papers of M. DE L'ISLE DE LA CROYERE's, who was himſelf far to the northward of Yakutſk, without hearing theſe noiſes; probably, therefore, it is much exaggerated, though one can ſcarcely ſuppoſe the whole to be fabulous.

[†] Muſſchenbroeck Introduct. § 2495. Beccaria dell' Elletriciſmo artif. et nat. p. 221.

But, in my opinion, the moſt remarkable analogy of all, and that which tends moſt to elucidate the origin of theſe meteors, is the direction of their courſe, which ſeems, in the very large ones at leaſt, to be conſtantly from or toward the north or north-weſt quarter of the heavens, and indeed to approach very nearly to the preſent magnetical meridian. This is particularly obſervable in thoſe meteors of late years whoſe tracks have been aſcertained with moſt exactneſs; as that of November 26, 1758, deſcribed by Sir JOHN PRINGLE; that of July 17, 1771, treated of by M. LE ROY; and this of the 18th of laſt Auguſt. The largeſt proportion of the other accounts of meteors confirm the ſame obſervation, even thoſe of a more early period *; nay, I think, ſome traces of it are per-

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p. 221. There is now working with Mr. NAIRNE, F. R. S. a perſon of the name of ARNOLD, who reſided ſeven years at Hudſon's Bay, the laſt three at Fort Henley. He confirms M. GMELIN's account of the fine appearance and brilliant colours of the northern lights, and particularly of their ruſhing noiſe, which he affirms he has very frequently heard, and compares it to the ſound produced by whirling round a ſtick ſwiftly at the end of a ſtring. He adds, that on converſing about this matter with a Swediſh watch-maker of the name of LIND, that perſon aſſured him, that he had heard a ſimilar noiſe in his own country: Mr. NAIRNE too, one time, at Northampton, when the northern lights were remarkably bright, is confident he perceived a hiſſing or whizzing ſound.

This hiſſing or ruſhing noiſe, as well as that attending meteors in their paſſage, ſuppoſing it in both caſes to be real, I would attribute to ſmall ſtreams of electric matter, running off to the earth from the great maſſes or accumulations of electricity, by which I ſuppoſe both meteors and the northern lights to be produced. Compare M. DE MAIRAN's Traité de l'Aurore Boréale, p. 126.

* See Phil. Tranſ. and Mem. de l'Acad. des Sciences, &c. I have found, of an earlier or later period, accounts of more than 40 different fire-balls. Of theſe above 20 are ſo deſcribed, that it is certain their courſe was in the above-mentioned direction; only 3 or 4 ſeem to have moved the contrary way; and with regard to the remainder, it is left doubtful, from the imperfect ſtate of the relations,

perceivable in the writings of the ancients *. Whether their motion ſhall be *from* the northern quarter of the heavens or *toward* it, ſeems nearly indifferent, as the numbers of thoſe going each way are not very unequal; I conſider them, in the former caſe, as maſſes of the electric fluid repelled, or burſting from the great collected body of it in the north; and, in the latter caſe, as maſſes attracted toward that accumulation; a diſtinction, probably, much the ſame in effect, as that of poſitive and negative electricity near the ſurface of the earth.

This tendency toward the magnetic meridian, however, ſeems to hold good only with regard to the largeſt ſort of fire-balls; the ſmaller ones move more irregularly, perhaps becauſe they come further within the verge of our atmoſphere, and are thereby more expoſed to the action of extraneous cauſes. That the ſmaller ſort of meteors, ſuch as ſhooting ſtars, are really lower down in the atmoſphere, is rendered very probable by their ſwifter *apparent* motion; perhaps it is this very circumſtance which occaſions them to be ſmaller, the electric

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tions. When we conſider that even the meteor of the 18th of Auguſt laſt was thought by *ſome* ſpectators to move ſouth-weſtward, it will rather appear ſurpriſing that ſo many of theſe accounts ſhould correſpond, than that a few of them ſhould differ.

* Aristotle (Meteor. lib. I. c. 6.) denies that comets, with which I take meteors to be confounded, are generated *only* in the north; which ſhews it to have been then the prevalent opinion, that they appeared moſt frequently in that quarter. Αλλα μην ουδε τȣτο αληθες, ὁτι εν τῳ προς αρκτον τοπῳ γινεται ὁ Κομητης μονον. So likewiſe Pliny (lib. II. c. 25.) Xiphias, Diſceus, Pitheus doliorum cernuntur figura, in concavo fumidæ lucis. Ceratias. Lampadias. Hippeus. Candidus Cometes. Omnes ferme *ſub ipſo ſeptentrione*, aliquâ ejus parte non certâ, ſed maxime in candidâ, quæ lactei circuli nomen accepit. And Seneca (Quæſt. Nat. lib. VII.) Placet ergo noſtris, Cometas, ſicut Tubas, Trabeſque, et alia oſtenta cœli, denſo aëre creari. Ideo *circa ſeptentrionem frequentiſſimè apparent*, quia illic plurimum eſt aëris pigri.

fluid

fluid being more divided in more reſiſting air. But as thoſe maſſes of electricity, which move where there is ſcarcely any reſiſtance, ſo generally affect the direction of the magnetic meridian, the ideas which have been entertained of ſome analogy between theſe two obſcure powers of nature, ſeem not altogether without foundation *.

If the foregoing conjectures be juſt, diſtinct regions are allotted to the electrical phænomena of our atmoſphere. Here below we have thunder and lightning, from the unequal diſtribution of the electric fluid among the clouds; in the loftier regions, whither the clouds never reach, we have the various gradations of falling ſtars; till beyond the limits of our crepuſcular atmoſphere the fluid is put into motion in ſufficient

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* It appears to me more rational to reſolve this analogy into a power of electricity to influence magnetiſm, than into a ſuppoſed ſimilarity of two fluids; as the former can be made evident by our artificial experiments, but there is no proof of the latter. When fire-balls, therefore, are ſaid to affect the magnetic meridian, I do not mean that they are drawn in that direction becauſe it is the line of magnetiſm, but rather that the magnetic poles of the earth are thrown into their preſent poſition, by the accumulation and action of that very electricity upon which the fire-balls depend. Should a change be produced by any cauſe in the place of this accumulation, or the ſtate of its motion, it is not improbable, that the main polarity would be given to other portions of the earth, whence a variation in the pointing of the compaſs would neceſſarily enſue. If Dr. FRANKLIN's hypotheſis be admitted, aſcribing the electrical ſtate of the polar atmoſphere to the cruſt of ice (a bad conductor) in thoſe regions, it follows, that ſhould ice form or be collected in one part more than in another, the atmoſphere there would become more highly electrical, and, in ſo far as the magnetiſm is given by electricity, the adjoining portion of the earth would acquire a ſtronger polarity. Now it is certainly worthy of remark, that ſince our firſt northern navigations, the coaſt of Weſt Greenland and its ſurrounding ſeas have become gradually more and more inacceſſible on account of ice, and that the magnetic needle all this time has been conſtantly changing its variation to the weſtward.

maſſes to hold a determined courſe, and exhibit the different appearances of what we call fire-balls; and probably at a ſtill greater elevation above the earth, the electricity accumulates in a lighter leſs condenſed form, to produce the wonderfully diverſified ſtreams and coruſcations of the *aurora borealis.*

I have the honour to be, with the greateſt reſpect,

SIR,

Your moſt obedient humble ſervant,

C. BLAGDEN.

END OF PART I. OF VOL. LXXIV.

PHILOSOPHICAL

TRANSACTIONS.

PART II.

PHILOSOPHICAL TRANSACTIONS,

OF THE

ROYAL SOCIETY

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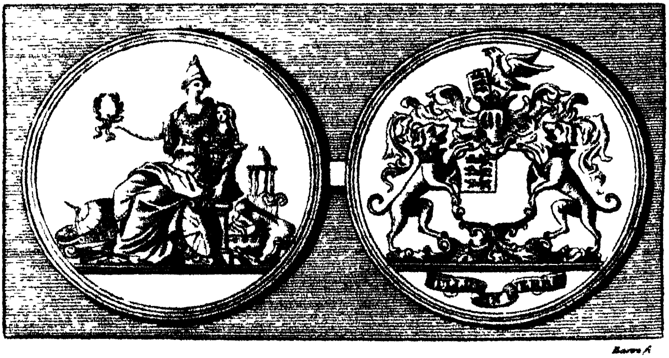

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Read March 11, 1784.

WHAT I have to offer on the ſubject of the remarkable appearances at the polar regions of Mars, as well as what relates to the inclination of the axis, the poſition of the poles, and the ſpheroidical figure of that planet, is founded on a ſeries of obſervations which I ſhall deliver in this paper; and after they have been given in the order they were made, it will be eaſy to ſhew, by a few deductions from them, that my theory of this planet is ſupported by facts which will ſufficiently authoriſe the concluſions I have drawn from them. For the ſake of better order and perſpicuity, however, I ſhall treat each ſubject apart, and begin with the remarkable appearances about the polar regions. The obſervations on them were made with a view to the ſituation and inclination of the axis of Mars; for to determine theſe we cannot conveniently uſe the ſpots on its ſurface, in the manner which is practiſed on the ſun. The quantities to be meaſured are ſo ſmall, and the obſervations of the center of Mars ſo precarious, and attended with ſuch difficulties (ſince an error of only a few ſeconds would be fatal) that we muſt have recourſe to other methods.

When I found that the poles of Mars were diſtinguiſhed with remarkable luminous ſpots *, it occurred to me, that we might obtain a good theory for ſettling the inclination and nodes of that planet's axis, by meaſures taken of the ſituation of thoſe ſpots. But, not to proceed upon grounds that wanted confirmation, it became neceſſary to determine by obſervation, how far theſe polar ſpots might be depended upon as permanent; and in what latitude of the globe of Mars they were ſituated; for, if they ſhould either be changeable, or not be at the very poles, we might be led into great miſtakes by overlooking theſe circumſtances. The following obſervations will aſſiſt us in the inveſtigation of theſe preliminary points.

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* A bright ſpot near the ſouthern pole, appearing like a polar zone, has alſo been obſerved by M. MARALDI. See Dr. SMITH's Optics, § 1094.

1777, April 17. 7 h. 50′. There are two remarkable bright ſpots on Mars. In fig. 1. tab. VI. they are marked *a* and *b*. The line AB expreſſes the direction of a parallel of declination. 10 feet reflector, 9 inches aperture, power 211 *.

10 h. 20′. They are both quite gone out of the diſk.

1779, This year, in all my obſervations on Mars, there is no mention of any bright ſpots, ſo that I believe there were none remarkable enough to attract my attention. However, as my view was particularly directed to the phænomena of this planet's diurnal rotation, it is poſſible I might overlook them.

1781, March 13. 17 h. 40′. 20 feet reflector. I ſaw a very lucid ſpot on the ſouthern limb of Mars of a conſiderable extent. See fig. 2.

June 25. 11 h. 36′. 7 feet reflector, power 227. Two luminous ſpots appeared at *a* and *b*, fig. 3.; *a* is larger than *b*.

12 h. 15′. With 460. *a* is thicker than *b*, but *b* is rather longer.

13 h. 12′. *a* is grown thicker, and *b* become thinner.

June 27. 11 h. 20′. The two lucid ſpots are on Mars.

June 28. 11 h. 15′. They are both viſible; *a*, fig. 4. is much thicker than *b*.

12 h. 55′. A line joining *a* and *b* does not go through the center.

June 30. 10 h. 48′. The ſpot *a* is viſible. fig. 5.

11 h. 35′. Both ſpots are to be ſeen.

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* Phil. Tranſ. vol. LXXI. p. 127. and fig. 17.

1781, July 3. 10 h. 54′. *a* ſeems to be larger than I have ſeen it, fig. 6.

11 h. 24′. *b* is not yet viſible, fig. 7.

12 h. 36′. I perceive part of *b*, fig. 8.

July 4. 12 h. 9. *a* is very full; *b* extremely thin, and barely viſible.

12 h. 18′. *a* and *b* are not quite oppoſite each other.

12 h. 49′. *b* is increaſed.

July 15. 9 h. 54′. *a* is viſible, fig. 9.

11 h. 35′. *b* inviſible.

12 h. 12′. *b* not to be ſeen.

July 16. 11 h. 9′. The bright ſpot *a* is very large.

July 17. 11 h. 15′. No other bright ſpot but *a*.

July 19. 13 h. 31′. *a* viſible.

July 20. 10 h. 3′. I ſuppoſe the bright ſpot *a* on Mars is, very nearly, the ſouth pole; which therefore muſt lie in ſight. There is no ſecond bright ſpot *b* viſible to night.

10 h. 56′ *b* not viſible; the night very fine.

July 22. 11 h. 14′. At *a* and *b*, fig. 10. are bright ſpots; *a* is larger than *b*. Moſt probably the ſouth pole is in view, and the north pole juſt hid from our ſight. If the ſpots are polar, or nearly ſo, then *a* muſt, on a ſuppoſition of the ſouth pole's being in view, appear larger than *b*; and if *b* extend a little more from the north pole one way than another, it muſt be ſubject to ſome change in its appearance from the revolution of Mars on its axis.

July 30. 9 h. 43′. Both ſpots viſible.

Auguſt 8. 10 h. 4′. Only *a* viſible, fig. 11.

Auguſt 17. 9 h. 21′. Only *a* in ſight.

1781, Auguſt 23. 8 h. 44′. *a* as uſual, and part of *b* viſible, fig. 12.

Sept. 7. The white ſpot *a* is very large.

1783, May 20. Mars has a ſingular appearance. At *a*, fig. 13. is the polar ſpot, which is bright, and ſeems to project above the diſk by its ſplendour, cauſing a break at *c*.

July 4. *a* is very bright.

July 23. 14 h. 45′. *a* is very lucid.

Auguſt 16. I ſaw the bright ſpot with the 20 feet reflector as uſual.

Aug. 26. The lucid ſpot on Mars is its ſouth pole, for it remains in the ſame place, while the dark equatorial ſpots perform their conſtant gyrations: it is nearly circular.

Aug. 29. The ſouth polar ſpot is in the ſame ſituation.

Sept. 9. As uſual.

Sept. 22. The ſouth polar ſpot is of a circular ſhape, and very brilliant and white. I had a beautiful and diſtinct view of it when it was about the meridian, and meaſured its little diameter in the equatorial direction of Mars. With a power of 932 it gave 1″ 41‴, and I ſaw it very diſtinctly. The outward edge of the ſpot came juſt up to the upper limb; a favourable hazineſs, taking off every troubleſome ray, gave me objects in general exceedingly well defined, eſpecially Mars.

Sept 23. 9 h. 55. The polar ſpot *a*, fig. 14. as uſual.

Sept. 24. The ſame.

1783, Sept. 25. 12 h. 30′. The bright ſouth polar ſpot *a*, fig. 15. ſeems to be fixed in its place, and goes nearly up to the margin of the diſk; it is perfectly round.

12 h. 55′. The track of the equatorial ſpots is incurvated, being convex towards the north, ſee *e*, *q*, fig. 23.: this confirms the white ſpot's being at the ſouth pole. With long attention I can perceive the edge of the diſk of Mars beyond the ſpot, extending about ¼ diameter of the ſpot.

Sept. 26. 12 h. 10′. The ſpot *a* is in a line with the center and the end of the hook, fig. 16.

Sept. 27, 28, 29. The ſpot as uſual.

Sept. 30. 10 h. 30′. The polar ſpot as in fig. 17.

Oct. 1. 9 h. 55′. I am inclined to think, that the white ſpot has ſome little revolution, and therefore is not with its center exactly at the pole of Mars; it is rather probable, that the real pole, though within the ſpot, may lie near the circumference of it, or one-third of its diameter from one of the ſides. A few days more will ſhew it, as I ſhall now fix my particular attention upon it.

10 h. 17′. The bright ſpot is certainly not ſo far upon the diſk as it uſed to be formerly, and is either reduced or has a ſmall motion; which of the two is the caſe will be ſeen in a few hours.

13 h. 3′. The bright ſpot has a little motion; for it is now come farther into the diſk.

I concluded now, in general, that none of the bright ſpots on Mars were exactly at the poles, though they could certainly not be far from them: for what has been juſt related of the

1ſt, 2d, and 3d of October 1783, ſhews plainly, that the appearance of the ſouthern ſpot *a* was a little affected by the diurnal motion of the planet; and the obſervations of the 3d and 4th of July 1781, ſhew alſo that the ſpot *b* could not be exactly at the north pole; and that, perhaps, the viſible branch of the latter extended pretty far towards the equator. However, the ſouth polar ſpot of the year 1783, being very ſmall and nearly round, afforded a good opportunity for determining its polar diſtance, by noting the different angles of poſition it aſſumed while Mars revolved on its axis; to this end many obſervations were taken at different hours of the ſame night, which will be found among the meaſures of the angles of poſition in the next diviſion of my ſubject. And ſince the different degrees of brilliancy, as well as the proportional apparent magnitude of the ſpot, would alſo contribute to the inveſtigation of this point, I continued my remarks on thoſe particulars, as follows.

1783, Oct. 2. 7 h. 59′. The bright ſpot near the ſouth pole is about half viſible.

Oct 4. 8 h. 0′. The polar ſpot ſeems to project above the diſk as formerly, and is very ſmall.

Oct. 5. 11 h. 13′. The ſpot is very ſmall, and ſeems actually to be in the circumference.

11 h. 30′. The ſpot is ſmall, and ſeems to be with its fartheſt ſide in the circumference of the diſk; or it may, perhaps, be partly beyond it, and therefore not all in ſight.

11 h. 50′. I ſee the ſpot much clearer than I did before.

13 h. 15′. The white ſpot is more in ſight, and of its uſual ſize, but does not ſeem much to change its poſition;

ſition; however, what change there is ſhews that it has been beyond the pole, as it appears to have been direct while the equatorial ſpots were retrograde.

1783, Oct. 9. 11 h. 48′. The white polar ſpot increaſes in ſize. At 10 h. 35′. it was as in fig. 18. but is now larger, and coming round towards that part of its orbit which is neareſt to us. See fig. 24.

Oct. 10. 6 h. 20′. I ſee no white polar ſpot; but the planet is too low for any obſervation to be depended on.

6 h. 55′. The white ſpot begins to be viſible; at leaſt I ſee it now, the planet being higher than before, fig. 19.

9 h. 55′. With 460, the white ſpot is conſiderably increaſed, and ſhews a circular form, fig. 20.

Oct. 11. 7 h. 46′. The bright ſpot is very viſible; the evening fine; with 278.

Oct. 16. 7 h. 7′. The ſpot is very luminous.

9 h. 55′. It ſeems rather lengthened; perhaps it may be arrived at the extreme of its parallel of declination.

Oct. 17. 7 h. 47′. The white ſpot *a*, fig. 21. is very bright.

13 h. 7′. It is leſs in appearance than it was in the beginning of the evening.

Oct. 23. 6 h. 46′. The bright ſpot is very large and luminous; I ſuppoſe it to be in the nearer parts of its little orbit.

7 h. 11′. It is ſituated as in fig. 22.

Oct. 24. 7 h. 1′. The white ſpot is very large.

Oct. 27. 8 h. 45′. It is very large and round.

Nov. 1. 7 h. 47′. The ſpot is round and bright.

1783, Nov. 11. The deficiency of light which occaſions Mars to appear gibbous, reaches over the ſouth polar ſpot towards the preceding limb, and hides it.

Nov. 14. Mars is gibbous, and the polar ſpot is thereby rendered inviſible.

Nov. 17. 6 h. 0′. The ſouth polar ſpot is under the falcated defect of light.

6 h 30′. I do not know whether there be not a faint glimpſe of the polar ſpot left; the weather is too bad to determine it.

I have added fig. 25. (tab. X.) to ſhew the connection of the 15th, 17th, 18th, 19th, 20th, 21ſt, and 22d figures, which complete the whole equatorial circle of appearances on Mars, as they were obſerved in immediate ſucceſſion. The center of the circle marked 17 is placed on the circumference of the inner circle, by making its diſtance from the center of the circle, marked 15, anſwer to the interval of time between the two obſervations, properly calculated and reduced to ſidereal meaſure. The ſame has been done with regard to the circles marked 18, 19, &c. And it will be found, by placing any one of theſe connected circles, ſo as to have its contents in a ſimilar ſituation with the figures in the ſingle repreſentation which bears the ſame number, that there is a ſufficient reſemblance between them; but ſome allowance muſt undoubtedly be made for the unavoidable diſtortions occaſioned by this kind of projection.

In order to bring theſe obſervations on the bright ſpots into one view, I have placed them at the circumference of three circles (ſee fig. 26, 27, 28. tab. VII. VIII. IX.) divided into degrees, repreſenting the parallels of declination in which they revolved about the poles of Mars. The diviſion of the circles marked 360 is where a ſpot paſſes that meridian of the planet which is turned towards the earth, and where, conſequently, it appears to us in its greateſt luſtre. The motion of the ſpot is according to the numbers 30, 60, 90, and ſo on to 360. In calculating the daily places of the ſpots I have uſed the ſidereal period of 24 h. 39′ 21″,67 determined in my paper on the rotation of Mars*; and have alſo made proper allowances for the alterations of the geocentric longitudes calculated from the ſituations of that planet given in the Nautical Almanack; by which means the ſidereal is reduced to a proper ſynodical period.

The following three tables contain the reſult of the calculations, and ſerve to explain the arrangement of the obſervations in the circles. In the firſt column are the times when the obſervations were made. In the ſecond, the ſidereal places of the ſpot in degrees and minutes. In the third column are the geocentric longitudes of Mars at the time of the obſervations. In the fourth, the neceſſary corrections on account of theſe different longitudes. In the fifth column are the corrected or ſynodical places of the ſpots; and, according to the numbers in that column, they are marked on the circles, where conſequently each ſpot is repreſented as it muſt have appeared to be ſituated at the time of obſervation.

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* Phil. Tranſ. vol. LXXI. p 134.

TABLE

T A B L E I.

Time of obſervation.				Sider. place.		Geoc. longit.			Correction.		Synod. place.	
	D.	H.	M.	D.	M.	S.	D.	M.	D.	M.	D	M.
June	25	11	36	350	31	9	24	35	+0	0	350	31
	25	12	15	0	0	9	24	35	+0	0	0	0
	25	13	12	13	52	9	24	34	−0	1	13	51
	27	11	20	357	28	9	24	12	−0	23	357	5
	28	11	15	316	40	9	24	1	−0	34	316	6
	30	10	48	290	56	9	23	38	−0	57	289	59
	30	11	35	302	23	9	23	38	−0	57	301	26
July	3	10	54	263	40	9	22	55	−1	40	262	0
	3	11	24	270	58	9	22	55	−1	40	269	18
	3	12	10	282	9	9	22	55	−1	40	280	29
	3	12	36	288	29	9	22	55	−1	41	286	48
	4	12	9	272	20	9	22	40	−1	55	270	25
	4	12	49	282	4	9	22	40	−1	55	280	9
	15	9	54	134	7	9	19	43	−4	52	129	15
	15	11	35	158	42	9	19	42	−4	53	153	49
	15	12	12	167	42	9	19	42	−4	53	162	49
	16	11	9	142	48	9	19	26	−5	9	137	39
	17	11	15	134	40	9	19	9	−5	26	129	14
	19	13	31	148	37	9	18	34	−6	1	142	36
	20	10	3	88	25	9	18	21	−6	14	82	11
	20	10	56	101	19	9	18	20	−6	15	95	4
	22	11	14	86	32	9	17	49	−6	46	79	46
	30	9	43	347	46	9	16	5	−8	30	339	16

TABLE II.

Time of obſervation.				Sider. place.		Geoc. longit.			Correction.		Synod. place.	
	D.	H.	M.	D.	M	S.	D.	M	D.	M.	D.	M.
June	25	11	36	86	51	9	24	35	+1	40	88	31
	25	12	15	96	20	9	24	35	+1	40	98	0
	25	13	12	110	12	9	24	34	+1	39	111	51
	28	11	15	53	0	9	24	1	+1	6	54	6
	30	10	48	27	16	9	23	38	+0	43	27	59
	30	11	35	38	43	9	23	38	+0	43	39	26
July	3	10	54	0	0	9	22	55	+0	0	0	0
	4	12	9	8	40	9	22	40	-0	15	8	25
	15	9	54	230	27	9	19	43	−3	12	227	15
	15	10	12	234	50	9	19	43	−3	12	231	38
	15	11	35	255	2	9	19	42	−3	13	251	49
	15	12	12	264	2	9	19	42	−3	13	260	49
	16	11	9	339	8	9	19	26	−3	29	235	39
	19	13	31	244	57	9	18	34	−4	21	240	36
	20	10	3	184	45	9	18	21	−4	34	180	11
	20	10	56	197	39	9	18	20	−4	35	193	4
	30	9	43	84	6	9	16	5	−6	50	77	16

TABLE

TABLE III.

Time of obſervation.			Sider. place.		Geoc longit.			Correction.		Synod. place.	
D.	H.	M.	D.	M.	S.	D.	M.	D.	M.	D.	M.
Sept. 25	13	30	6	32	0	9	54	+6	44	13	16
Oct. 1	10	17	262	5	0	8	6	+4	56	267	1
1	13	3	302	29	0	8	5	+4	55	307	24
2	7	59	218	55	0	7	50	+4	40	223	25
4	8	0	200	0	0	7	15	+4	5	204	5
4	8	46	211	12	0	7	15	+4	5	215	17
5	11	13	237	23	0	6	55	+3	45	241	8
5	11	30	241	31	0	6	55	+3	45	245	16
5	11	50	246	23	0	6	55	+3	45	250	8
5	13	15	267	4	0	6	54	+3	44	270	48
5	14	0	278	1	0	6	53	+3	43	281	44
7	8	20	176	8	0	6	23	+3	13	179	21
7	10	5	201	41	0	6	22	+3	12	204	53
7	11	50	227	14	0	6	21	+3	11	230	25
9	11	48	207	35	0	5	49	+2	39	210	14
10	6	55	126	42	0	5	37	+2	27	129	9
10	7	50	140	5	0	5	36	+2	26	142	31
10	9	55	170	30	0	5	34	+2	24	172	54
10	12	11	203	36	0	5	33	+2	21	205	57
16	7	7	72	9	0	4	15	+1	5	73	14
16	7	46	81	39	0	4	15	+1	4	82	43
16	9	55	113	2	0	4	14	+1	4	114	6
17	7	47	72	19	0	4	3	+0	53	73	12
17	13	7	150	11	0	4	0	+0	50	151	1
23	6	46	0	0	0	3	10	−0	0	0	0
24	7	1	354	0	0	3	2	−0	8	353	52

From the appearance and diſappearance of the bright north polar ſpot in the year 1781, we collect that the circle of its motion, repreſented by fig. 26. was at ſome conſiderable diſtance from the pole. By a calculation, made according to the principles hereafter explained, its latitude muſt have been about 76° or 77° north; for I find that, to the inhabitants of Mars, the declination of the ſun, June 25. 12 h. 15′ of our time, was about 9° 56′ ſouth*; and the ſpot muſt have been at leaſt ſo far removed from the north pole as to fall a few degrees within the enlightened part of the diſk, to become viſible to us.

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* See p. 259. and 260.

The ſouth pole of Mars could not be many degrees from the center of the large bright ſouthern ſpot of the year 1781, whoſe courſe is traced in fig. 27; though the ſpot was of ſuch a magnitude as to cover all the polar regions farther than the 70th or 65th degree, and in that part which was on the meridian July 3, at 10 h. 54′, perhaps a little farther.

In the next diviſion of our ſubject will be ſhewn, that the inclination and poſition of the axis of Mars are ſuch, that the whole circle, fig. 28. (which will appear to be in about 81° 52′ of ſouth latitude on the globe of Mars) was in view all the time the obſervations on the bright ſouth polar ſpot of the year 1783, which are marked upon it, were made, but in ſo oblique a ſituation as to be projected into a very narrow ellipſis. See fig. 24. where mn is the little ellipſis in which the ſpot a revolved about the pole. Hence then we may eaſily account for the obſerved magnitude and brightneſs of the ſpot Oct. 23, 24, and 27. when it was expoſed to us in its meridian ſplendour. Its ſituations Oct. 16. and 17. on one extreme of the parallel, as well as thoſe of Oct. 5. and Nov. 1. on the other, gave us alſo a bright view of it: and when we paſs over to that half of the circle which lies beyond the pole, the much greater obliquity into which the ſpot muſt there be projected will perfectly account for its being ſmaller at 13 h. 7′ of Oct. 17. than at 7 h. 47′ of the ſame evening. It will alſo explain its ſmallneſs Oct. 4. and its increaſe Oct. 9. We ſhall have occaſion hereafter to recur to the ſame figure, ſo that I take no notice at preſent of the angles of poſition which are marked upon it.

Of the direction or nodes of the axis of Mars, *its inclination to the ecliptic, and the angle of that planet's equator with its own orbit.*

From the foregoing article we may gather; that the bright polar ſpots on Mars are the moſt convenient objects for determining the ſituation of the axis of this planet; I ſhall therefore collect, in one view, all the meaſures I have taken of theſe ſpots for that purpoſe. Before I conſtructed a micrometer for taking the angle of poſition, I uſed to draw a line through the figure delineated of Mars to repreſent the parallel of declination; in a few of my firſt obſervations, therefore, I can only take the ſituation of the polar ſpots from ſuch drawings, and of conſequence no great accuracy in the angles, as to the exact number of degrees, can be expected.

1777, April 17. 7 h. 50′. A line drawn through the middle of the two bright polar ſpots *a* and *b*, fig. 1. makes an angle of about 63°, with a parallel of declination AB; the ſouthern ſpot preceding and the northern following.

My reaſon for chuſing a line drawn through both the ſpots rather than through one of them and the center is, firſt, that they were not ſituated quite oppoſite each other, and therefore, unleſs other obſervations had pointed out which was moſt polar, I ſhould evidently run the greater riſk in fixing on one of them in preference to the other. In the next place, we find by the ſecond obſervation, page 235. that in two hours and a half both ſpots were intirely gone out of the diſk. This

plainly

plainly denotes, that they were both in the ſame half of a ſphere orthographically projected, and divided by a plane paſſing through the axis of Mars and the eye, but that neither of them were polar. Now, a line drawn through two points not far from oppoſite each other, both in the ſame hemiſphere, and both removed from the poles of it, muſt approach more to a paralleliſm with the axis, than a line drawn through either of them and the center.

1779, May 9. There being no bright ſpots by which to judge of the poſition of the poles, it is eſtimated from a well known dark equatorial ſpot, with a line drawn through the figure to denote a parallel of declination. By very rough eſtimation it is about 42° ſouth preceding.

May 11. The ſame figure, being drawn again in another ſituation, and alſo with a line giving a parallel of declination, points out, by the ſame rough eſtimation, 62° ſouth preceding.

1781, June 25. 11 h. 35′. The poſition of the ſpots *a* and *b*, fig. 3. with regard to a parallel of declination, meaſured with a micrometer 74° 32′. The ſpot *a* was ſouth preceding, and *b* north following.

July 15. 10 h. 12′. The angle of poſition, of the center of the ſpot *a*, fig. 9. through the center of the diſk, 74° 18′ ſouth preceding.

1783, Auguſt 16. Poſition of the ſpot *a*, 64° ſouth following the center; but as the planet is not full, the center becomes dubious, and the meaſure therefore may not be quite accurate, though taken with a 20 feet reflector; power 200.

783, Sept. 9. Poſition of the ſuppoſed ſouth pole of Mars 65° 12′ ſouth following; 7 feet reflector; power 460.

Sept. 22. Poſition of the ſame 52° 9′ ſ. following; 460.

Sept. 25. 13 h. 30′. Poſition of the ſouth polar ſpot 56° 27′. very accurately taken, by biſecting the diſk of Mars through the bright ſpot, and ſuppoſing the planet now near enough the oppoſition to induce no material error. Hitherto I have taken it through a ſuppoſed center by endeavouring to allow a little for what I thought the deficiency in the diſk; but not to-night.

Oct. 4. 8 h. 46′. Poſition of the ſpot 51° 21′; Mars too low and hazy to depend much on the meaſure with ſo high a power as 460.

Oct 5. The motion of the polar ſpot being now ſtrongly ſuſpected, or rather already known, I took the following meaſures, by way of diſcovering its quantity.

11 h. 50′. Poſition very exactly taken 50° 6′ ſ. following.

14 h. 0′. Poſition of the ſpot 49° 45′.

Oct. 7. 8 h. 20′. Poſition 55° 12′. In order to ſee how far this meaſure might be truſted to, I ſet 49° 36′ in the micrometer, which was evidently too ſmall; next I took 51° 36′, which was alſo too ſmall; after this, I took a new meaſure, and found 55° 24′, which appeared to me very exact. 10 h. 5′. The poſition now was 53°. 11 h. 50′. It meaſured 52° 12′. As there is nothing to diſtinguiſh the center, it is extremely difficult to pleaſe one's ſelf in bringing the ſpot into a line with it.

1783, Oct. 10. 7 h. 50′. Poſition of the polar ſpot 57° 12′; with 460, very accurate. I tried a few parts leſs of the micrometer, but found the meaſure too little. I ſee pretty diſtinctly, but the air is tremulous.

9 h. 55′. Poſition 52° 42′; very diſtinct.

12 h. 11′. Poſition 46° 30′; I ſee not quite ſo well now as I could wiſh.

14 h. 1′. Poſition 44° 12′; but liable to great uncertainty, on account of tremulous air; it becomes more difficult to diſtinguiſh the center when the planet is not perfectly defined.

Oct. 16. 7 h. 7′. Poſition 63° 9′. By way of trial I ſet 59° 36′, which was too ſmall; alſo 60° 24′ was too ſmall; again, 61° 24′ was not large enough. Then, taking a freſh meaſure, I found it 62° 48′, which I thought right.

9 h. 55′. I took three meaſures, and thought the third, which was 65° 0′, the beſt of all, for I ſaw the planet and the ſpot remarkably well.

Oct. 27. 8 h. 45′. Poſition of the polar ſpot 59° 30′. I took three other meaſures, of which 60° 39′ appeared to me the beſt; it was taken with long attendance and many changes and trials of the wires in different poſitions; but the gibboſity of Mars is ſuch, that meaſures of the ſituation of the ſpot are now no longer to be depended on.

Theſe poſitions, I believe, will be ſufficient for the purpoſe of ſettling the latitude of the polar ſpots, and thereby obtaining a correct meaſure of the ſituation of the real pole. I have referred thoſe of the ſouth polar ſpot of the year 1783 to the ſame circle which contains the obſervations that were made on the apparent brightneſs and magnitude of that ſpot, that they may be compared together. (See fig. 28.) The agreement of the meaſures, and the phænomena attending the motion of the ſpot, are ſufficient to point out the meridian of the circle; for which, from a due conſideration of theſe circumſtances, I have fixed on the place where the ſpot was Oct. 10. 6 h. 46′.

Of the angles collected in fig. 28. we find 65° 0′ the largeſt, and 49° 45′ the ſmalleſt; but, on account of the different ſituation of the earth and Mars, the angle meaſured 7′ leſs Oct. 16. than it would have done had the planets remained in the places they were in Oct. 5. when the other meaſure was taken. This being added, we have 65° 7′. The difference between the two poſitions is 15° 22′. Now, the conſtruction of fig. 28. being admitted, we ſee that the angles were nearly taken at the oppoſite extremes of the circle in which the ſpot moved. However, by the 5th column of Tab. III. Oct. 5. we have the ſituation of the ſpot in the circle with reſpect to the meridian 281° 44′, and Oct. 16. 114° 6′: therefore the ſouth polar diſtance of the center of the ſpot is found, by taking half the ſum of the ſines of theſe angles to radius, as 7° 41′ (half of 15° 22′) to a fourth number, which is 8° 8′; and the latitude of the circle, in which the ſpot moved about the pole, therefore is 81° 52′ ſouth. This being determined, we have the following correction for the angles of poſition: radius is to ſine of the angular diſtance of the ſpot from the meridian as 8° 8′ to the required quantity. This muſt be added or ſubtracted, according as the caſe requires; and thereby we ſhall have the poſition of the true pole from any one of the meaſures.

I ſhall now apply the above to determine the ſituation of the axis of Mars. To this end, we ſee that, in the firſt place, the meaſures muſt be corrected for the latitude of the ſpot; next, they muſt be reduced to a heliocentric obſervation, which will alſo correct them from the difference occaſioned by the different ſituation of the planets when they were taken. This being done, we may ſelect two obſervations at a proper diſtance; from which, by trigonometry, we ſhall have the node and inclination of the axis. When theſe elements are obtained, it will be eaſy to ſee how other obſervations agree with them; which will afford the means of correcting or verifying the former calculations.

Let T, fig. 29. (tab. X) be the earth; ♋ Q *q* ♑ the ecliptic as ſeen from T; P the point of the heavens towards which the north pole of the earth is directed; M the place of the orbit of Mars *μ m* M, where an obſervation of the poles of that planet has been made, which is to be reduced to its heliocentric meaſure. And, firſt, ſuppoſe it to have been made at the time of the oppoſition of that planet. Then, the place M or Q in the ecliptic being given, we have the ſides Q ♋, ♋ P; whence the angle Q, of the right-angled triangle P ♋ Q, is found. This being added to, or taken from, the obſerved angle of poſition of the axis of Mars, according to circumſtances eaſily to be determined, reduces it to its heliocentric poſition. But if this obſervation was not made at the time of an oppoſition, but at ſome other place *m*, a ſecond correction is to be applied in the following manner.

Let the angle *q*, of the triangle P ♋ *q*, be found as before, and properly applied to the poſition of the axis of Mars now at *m*; then make the angle *m* S *μ*, at the ſun S, equal to the angle S *m* T, and *μ* will be the heliocentric place, where the angle of poſition, when ſeen from S, will appear to be as it was found at *m*, after the application of the firſt correction:

for Sμ being parallel to Tm, and ſuppoſing the axis of Mars to preſerve its paralleliſm while it moves from m to μ, appearances of Mars at μ to an eye at S, muſt be the ſame as they are at m to an eye at T.

The following table contains the reſult of calculations relating to the angles of fig. 28. In the firſt column are the times when the obſervations were made. In the ſecond, the angles as they were taken. In the third column are the quantities of the angles Q, q, calculated from the geocentric longitudes contained in the third column of the third table. In the fourth column are the corrections for the ſituation of the ſpot in the circle of latitude obtained from the ſines of the angles in the fifth column of the third table. In the fifth are the corrections requiſite on account of the change of ſituation of the planets, during the interval between the ſeveral days on which the meaſures were taken; theſe are obtained from the third column of this table, and I have aſſumed the 4th of October, as being the obſervation neareſt the oppoſition, to which I have reduced the other meaſures. In the ſixth column are the angles of the ſecond, corrected by the quantities contained in the fourth and fifth columns, applied according to their ſigns.

TABLE IV.

Time of obſervation.			Angles taken.		Angle Q.		Firſt correction.		Second correct.	Angles corrected.	
D.	H.	M.	D.	M.	D.	M.	D.	M.	M.	D.	M.
Sept. 25	13	30	56	27	+23	10	−1	52	−8	54	27
Oct. 4	8	46	51	21	+23	18	+4	42	−0	56	3
5	11	50	50	6	+23	19	+7	39	+1	57	46
5	14	0	49	45	+23	19	+7	59	+1	57	45
7	8	20	55 55	12 24	+23	21	−0	7	+2	55 55	7 19
7	10	5	53	0	+23	21	+3	26	+3	56	29
7	11	50	52	12	+23	21	+6	16	+3	58	31
10	7	50	57	12	+23	22	−4	57	+4	52	19
10	9	55	52	42	+23	22	−1	7	+4	51	39
16	7	7	63 62	9 48	+23	25	−7	47	+7	55 55	29 8
16	9	55	65	0	+23	25	−7	23	+7	57	45

As we have no particular reaſon to ſelect one meaſure rather than another, a mean of all the 13 will probably be neareſt the truth; ſo that by theſe obſervations, which, as we ſaid before, are reduced to the 4th of October, 1783, we find the poſition of the axis of Mars that day to have been 55° 41′ ſouth following.

From the appearances of the ſouth polar ſpot in 1781, repreſented fig. 27. we may conclude, that its center was nearly polar. We find it continued viſible all the time Mars revolved on its axis; and, to preſent us generally with a pretty equal ſhare of the luminous appearance, a ſpot which covered from 45° to 60° of a great circle on the globe of Mars could not have any conſiderable polar diſtance: however, a ſmall correction in the angle of poſition ſeems to be neceſſary, which ſhould be taken from the meaſure of the 15th of July, becauſe that branch of the ſpot which probably extended fartheſt towards the equator, was then in the *following* quadrant. The meaſure of both the ſpots on June the 25th, 1781, is ſtill more to be depended on, as giving us very nearly the poſition of the true pole; for it appears evident from the phænomena of the bright north-polar ſpot in fig. 26. that that ſpot was in the meridian when the meaſure was taken, while the ſouthern ſpot was in the *preceding* quadrant near its greateſt limit. Now, ſince an angle at the circumference of a circle is but half the angle at the center, when the arches which ſubtend theſe angles are equal, the correction neceſſary to be applied to the meaſure taken through the two ſpots will be but one half of the correction which would have been requiſite had it been taken through the center; therefore, in order to reduce this to the condition of the former, we may ſuppoſe it to have been taken through the center of Mars when the ſpot was only 30, or 150 degrees from the meridian. It is alſo neceſſary to add 1° 54′ to the angle of July 15, which it would have meaſured more had the planets remained where they were June 25. This done, we may have the polar diſtance of the center of the ſpot as before. Half the ſum of the ſines (of 231° 38′ and 150°) to radius, as 50′ (half the difference between 74° 32′ and 76° 12′) to a fourth number, which is 1° 18′.

I ſhould obſerve here, that the meaſures of the angle of poſition would be too large before the ſpot came to the meridian, and too ſmall afterwards, the axis of Mars being ſouth preceding; whereas, in fig. 28. they would be too ſmall before, and too large after, the meridian paſſage, the pole being ſouth following.

Theſe two obſervations arranged as thoſe in the fourth table, and reduced to the time of the 25th of June, will ſtand as follows.

TABLE

TABLE V.

Time of obſervation.			Angles taken.		Angle Q.		Firſt correction.	Second correct.		Corrected Angle.	
D.	H.	M.	D.	M.	D.	M.		D.	M	D.	M.
June 25	11	36	74	32	−10	14	+ half of 1° 18′	−0	0	75	11
July 15	10	12	74	18	− 8	20	−1 1	+1	54	75	11

I am to remark, that we have here admitted both meaſures as equally good; and that, therefore, the reſult is a mean of them both, and ſhews the axis of Mars, June 25, 1781, to have been 75° 11′ ſouth preceding.

Our next buſineſs will be to reduce theſe two geocentric obſervations to a heliocentric meaſure. This is to be done, as we have ſhewn before, by a calculation of the angle Q, fig. 29. The reſult of it ſhews, that 10° 14′ are to be ſubtracted from the mean corrected angle of poſition, reduced to June 25, 1781, and 23° 18′ to be added to the angle which is the corrected mean of 13 meaſures, reduced to Oct. 4, 1783. Hence we learn, that on thoſe days and hours, when the heliocentric places of Mars were 9 s. 24° 35′, and 0 s. 7° 15′ (which would happen about July 18, 1781, and Sept. 29, 1783) an obſerver placed in the ſun would have ſeen, on the former, the axis of Mars inclined to the ecliptic 64° 57′, the north pole being towards the left; and on the latter, he would have ſeen the ſame axis inclined to the ecliptic 78° 59′, the north pole being then towards the right.

The firſt concluſion we may draw from theſe principles is, that the north pole of Mars muſt be directed towards ſome point of the heavens between 9 s. 24° 35′ and 0 s. 7° 15′; becauſe the change of the ſituation of the pole from left to right, which happened in the time the planet paſſed from one place to the other, is a plain indication of its having gone through the node of the axis. Next, we may alſo conclude, that the node muſt be conſiderably nearer the latter point of the ecliptic than the former; for, whatever be the inclination of the axis, it will be ſeen under equal angles at equal diſtances from the node.

But, by a trigonometrical proceſs of ſolving a few triangles, we ſoon diſcover both the inclination of the axis, and the place where it interſects the ecliptic at rectangles (which, for want of a better term, I have perhaps improperly called its node). Accordingly I find, by calculation, that the node is in 17° 47′ of Piſces, the north pole of Mars being directed towards that part of the heavens; and that the inclination of the axis to the ecliptic is 59° 42′.

We ſhall now compare the obſervations of an earlier date with theſe principles, to ſee how far they agree. Some of the particulars and calculations relating to them are as follow.

TABLE VI.

Times of Obſervation.				Eſtimations.	Geoc. longit.			Angle Q.		2d correct.
	D.	H.	M.	D.	S.	D.	M.	D.	M.	
1779, May	9	12	0	42	7	22	20	+14	45	+ 0
May	11	12	0	62	7	21	40	+15	11	+ 26
1777, Apr.	17	7	50	63	6	3	34	+23	26	

May the 9th, 1779, as we have ſeen, the angle of poſition was roughly eſtimated at 42°, and May 11. at 62°. The great diſagreement of theſe coarſe eſtimations is undoubtedly owing to the very different ſituation of the dark ſpot from which they were taken; however, ſince we do not mean to uſe theſe obſervations in our calculations, they may ſuffice in a general way to ſhew, that the axis of Mars was actually about that time in ſuch a ſituation as our principles give it: for, reducing the two poſitions to the 9th of May, that of the 11th, from an allowance of 26′ for the ſituation of the planets, will become 62° 26′; and a mean of the two, 50° 13′ ſouth preceding; which, reduced to a heliocentric obſervation, gives 66° 30′, the north pole lying towards the left. Now, on calculating from the poſition of the node and inclination of the axis before determined, we find, that the heliocentric angle was 62° 49′, the north pole pointing towards the left; and a nearer agreement with theſe principles could hardly be expected from eſtimations ſo coarſe. If we go to the year 1777, and take the poſition of the two bright ſpots obſerved the 17th of April, we have 63° ſouth preceding; this, reduced to a heliocentric quantity, gives 86° 26′ of inclination, the north pole being to the left. By calculating we find, that that pole was then actually 81° 27′ inclined to the ecliptic, and pointed towards the left as ſeen from the ſun.

The inclination and ſituation of the node of the axis of Mars with reſpect to the ecliptic being found may thus be reduced to that planet's own orbit. Let EC, fig. 30. (tab. X.) be a part of the ecliptic; OM part of the orbit of Mars; PEO a line drawn from P, the celeſtial pole of Mars, through E, that point which has been determined to be the place of the node of the axis of Mars in the ecliptic, and continued to O where it interſects the orbit of Mars. Now, if according to Mr. DE LA LANDE we put the node of the orbit of Mars for 1783, in 1 s. 17° 58′, we have from the place of the node of the axis (that is, 11 s. 17° 47′) to the place of the node of the orbit, an arch EN of 60° 11′; in the triangle NEO, right-angled at E, there is alſo given the angle ENO, according to the ſame author, 1° 51′, which is the inclination of the orbit of Mars to the ecliptic. Hence we find the angle EON 89° 5′, and ſide ON 60° 12′. Again, when Mars is in the node of its orbit N, we have, by calculation from our principles, the angle PNE = 63° 7′, to which, adding the angle ENO = 1° 51′, we have PNO = 64° 58′; from which two angles PON and PNO with the diſtance ON, we obtain the inclination of the axis of Mars, and place of its node with reſpect to that planet's own orbit; the inclination being 61° 18′, and the place of the node of the axis 58° 31′ preceding the interſection of the ecliptic with the orbit of Mars, or in our 19° 28′ of Piſces.

Being thus acquainted with what the inhabitants of Mars will call the obliquity of their ecliptic, and the ſituation of their equinoctial and ſolſtitial points, we are furniſhed with the means of calculating the ſeaſons on Mars; and may account, in a manner which I think highly probable, for the remarkable appearances about its polar regions.

But firſt it may not be improper to give an inſtance how to reſolve any query concerning the martial ſeaſons. Thus, let it be required to compute the declination of the Sun on Mars, June 25, 1781, at midnight of our time. If ♈ ♉ ♊ ♋, &c. fig. 31. (tab. X.) repreſent the ecliptic of Mars, and ♈ ♋ ♎ ♑ the ecliptic of our planet, A*a*, *b*B, the mutual interſection of the martial and terreſtrial ecliptics, then there is given the heliocentric longitude of Mars, ♈*m* = 9 s. 10° 30′; then taking away ſix ſigns, and ♎ *b*, or ♈*a* = 1 s. 17° 58′, there remains *bm* = 1 s. 22° 32′. From this arch, with the given inclination, 1° 51′, of the orbits to each other, we have coſine of inclination to radius, as tangent of *bm* to tangent of BM = 1 s. 22° 33′. And taking away B♈ = 1 s. 1° 29′, which is the complement to ♑ B (or ♋ A, already ſhewn to be 1 s. 28° 31′) there will remain ♈M = 0 s. 21° 4′, the place of Mars in its own orbit *; that is, on the time abovementioned, the ſun's longitude on Mars will be 6 s. 21° 4′, and the obliquity of the martial ecliptic 28° 42′ being alſo given, we find, by the uſual method, the ſun's declination 9° 56′ ſouth.

The analogy between Mars and the earth is, perhaps, by far the greateſt in the whole ſolar ſyſtem. Their diurnal motion is nearly the ſame; the obliquity of their reſpective ecliptics, on which the ſeaſons depend, not very different; of all the ſuperior planets the diſtance of Mars from the ſun is by far the neareſt alike to that of the earth: nor will the length of the martial year appear very different from that which we enjoy, when compared to the ſurpriſing duration of the years of Jupiter, Saturn, and the Georgium Sidus. If, then, we find that the globe we inhabit has its polar regions frozen and covered with mountains of ice and ſnow, that only partly melt when alternately expoſed to the ſun, I may well be permitted to ſurmiſe that the ſame cauſes may probably have the ſame effect on the globe of Mars; that the bright polar ſpots are owing to the vivid reflection of light from frozen regions; and that the reduction of thoſe ſpots is to be aſcribed to their being expoſed to the ſun. In the year 1781, the ſouth polar ſpot was extremely large, which we might well expect, ſince that pole had but lately been involved in a whole twelvemonth's darkneſs and abſence of the ſun; but in 1783 I found it conſiderably ſmaller than before, and it decreaſed continually

from

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* If no very great accuracy be required, we may add 3 s. 10° 34′ to any given place of our ecliptic, which will at once reduce it to what it ſhould be called on the orbit of Mars, and will always be true to within a minute.

from the 20th of May till about the middle of September, when it ſeemed to be at a ſtand. During this laſt period the ſouth pole had already been above eight months enjoying the benefit of ſummer, and ſtill continued to receive the ſun-beams; though, towards the latter end, in ſuch an oblique direction as to be but little benefited by them. On the other hand, in the year 1781, the north polar ſpot, which had then been its twelve-month in the ſun-ſhine, and was but lately returning to darkneſs, appeared ſmall, though undoubtedly increaſing in ſize. Its not being viſible in the year 1783 is no objection to theſe phænomena, being owing to the poſition of the axis, by which it was removed out of ſight; moſt probably, in the next oppoſition we ſhall ſee it renewed, and of conſiderable extent and brightneſs; as, by the poſition of the axis of Mars, the ſun's ſouthern declination will then be no more than 6° 25′ on that planet.

Of the ſpheroidical figure of Mars.

That a planetary globe, ſuch as Mars, turning on an axis, ſhould be of a ſpheroidical form, will eaſily find admittance, when two familiar inſtances in Jupiter and the earth, as well as the known laws of gravitation and centrifugal force of rotatory bodies, lead the way to the reception of ſuch doctrines. So far from creating difficulties or doubts, it will rather appear ſingular, that the ſpheroidical form of this planet, which the following obſervations will eſtabliſh, has not already been noticed by former aſtronomers; and yet, reflecting on the general appearances of Mars, we ſoon find that opportunities for making obſervations on its real form cannot be very frequent: for, when it is near enough to view it to an advantage, we ſee it generally gibbous, and its oppoſitions are ſo ſcarce, and of ſo ſhort a duration, that in more than two years time we have not above three or four weeks for ſuch obſervations. Beſides, aſtronomers being already uſed to ſee this planet generally diſtorted, the ſpheroidical form might eaſily be overlooked.

Obſervations relating to the polar flattening of Mars.

1783, Sept 25. 9 h. 50′. I can plainly ſee that the equatorial diameter of Mars is longer than the polar. Meaſure of the equatorial diameter 21″ 53‴; of the polar diameter 21″ 15‴ *full meaſure*, that is, certainly not too ſmall. The wires were ſet as outward tangents to the diſk, and the zero, as well as the meaſures, were taken by the light of Mars.

Sept. 28. 14 h. 25′. I ſhewed the difference of the polar and equatorial diameters of Mars to Mr. WILSON, Aſſiſtant Profeſſor of Aſtronomy at Glaſgow. He ſaw it perfectly well, ſo as to be entirely convinced it was not owing to any defect or diſtortion occaſioned by the eye lens; and, becauſe I wiſhed him to be ſatisfied of the reality of the appearance, while he was obſerving, I reminded him of ſeveral well known precautions; ſuch as cauſing the planet to paſs directly through the center of the field of view, and judging of its figure at the time when it was moſt diſtinct and beſt defined, and ſo forth.

Sept. 29. I ſhewed the difference of the polar and equatorial diameters of Mars to Dr. BLAGDEN and Mr. AUBERT. Dr. BLAGDEN not only ſaw it immediately,

diately, but thought the flattening almoſt as much as that of Jupiter. Mr. AUBERT alſo ſaw it very plainly, ſo as to entertain no manner of doubt about the appearance.

As we cannot take too many opportunities of confirming our own obſervations by the eyes of other obſervers, I eſteemed it a very fortunate circumſtance to have the honour of a viſit from theſe gentlemen at ſo particular a time, Mars being this day within 37 hours of the oppoſition, and yeſterday when Mr. WILSON ſaw it, within about two days and a half.

1783, Sept. 30. 10 h. 52′. The difference in the diameters of Mars is very evident and conſiderable.

Meaſure of the equatorial diameter	22″ 9‴	with 278.
Second meaſure - -	22″ 31‴	full large.
Polar diameter very exact -	21″ 26‴.	

Oct. 1. 10 h. 50′. I took meaſures of the diameters of Mars with my 20-feet reflector. The equatorial meaſured 103 parts of the micrometer; the polar 98. The value of the diviſions in ſeconds and thirds not being well determined, on account of ſome late change in the focal length of the ſeveral 20-feet object metals I uſe, we have only from theſe meaſures the proportion of the diameters as 103 to 98.

13 h. 15′. Every circumſtance being favourable, I took the following meaſures of the diameters of Mars with my 7-feet reflector, and a diſtinct power of 625.

Equatorial diameter	22″ 12‴	narrow meaſure.
	22″ 46‴	rather full.
	22″ 35‴	exact.

Polar diameter 21″ 24‴

21″ 33‴ very exact.

I ſaw Mars perfectly well all the time I meaſured, with all its figures upon the diſk appearing diſtinctly; and, I think, theſe meaſures may be depended upon better than any I have yet taken.

1783, Oct 5. 14 h. 0′. The difference of the diameters is very ſenſible.

Oct. 7. 9 h. 43′. The flattening of the poles is very viſible.

13 h. 40′. I turned my Newtonian 7-feet reflector one quarter round, ſo as to bring the place to look in at to the bottom; and, as well as the uneaſy poſture would allow, I ſaw the flattening of the poles the ſame as when I looked in at the ſide; power 460.

14 h. 30′. With a 3½ feet achromatic teleſcope and a ſingle eye lens, I ſaw the difference of the polar and equatorial diameters very plainly.

Oct. 9. 8 h. 40′. I turned my reflector 90° round, ſo as now to look in at the upper end, but ſaw not the leaſt difference in appearances; for, returning it again immediately to its uſual poſition, in both caſes the equatorial diameter appeared a little longer than the other; power 278, and the evening fine.

I turned the great ſpeculum one quadrant in its cell, but appearances were not in the leaſt altered; the equatorial diameter ſtill was a little longer than the polar one.

I tried a very fine new object ſpeculum, and found alſo the equatorial diameter a little longer than the polar one.

1783, Oct. 9. 10 h. 47′. The flattening at the poles very viſible.

Oct. 10. 9 h. 55′. A little of the polar flattening is viſible, ſo as to admit of no doubt; power 460, very diſtinct.

11 h. 32′. Mars viſibly flattened, but not much; the achromatic ſhews it alſo.

11 h. 42′. The diſk of Mars is viſibly ſpheroidical.

Oct. 11. 7 h. 37′. Mars is plainly gibbous, therefore meaſures and eſtimations of the diameters muſt for the future be improper.

11 h. 12′. It is rather difficult to ſay of what ſhape Mars is now, for it is partly flattened and partly gibbous; but the gibbous ſide not being quite in the polar direction of Mars, this produces altogether an odd mixture of ſhapes: however, upon the whole, the polar diameter is ſtill rather the ſmalleſt.

11 h. 13′. The *preceding* ſide of Mars ſhews the flattening of the poles, while the *following* is terminated by an elliptical arch.

Oct. 12. 11 h. 12′. The flattening upon the whole is viſible.

Oct. 17. 13 h. 7′. The effect of gibboſity is ſcarcely equal to the flattening; or, upon the whole, the planet is ſtill rather broader over the equator than over the poles.

Nov. 1. 7 h. 56′. The ſemi-diſk, which is *full*, is evidently part of an oblate ſpheroid; but, to an eye not attentively looking for it, and knowing the ſhape and exact ſituation of the poles of Mars, this would probably not appear.

1783, Nov. 10. 9 h. 30′. The gibboſity of Mars is now ſuch, that the polar diameter is conſiderably longer than the equatorial; but the deficiency not being exactly from pole to pole, makes the diſk of a crooked, irregular figure, and renders preciſion in this eſtimation impoſſible; otherwiſe the phaſe of Mars would have made a pretty good micrometer upon the equatorial diameter, and it was with ſuch a view I had directed my attention to this circumſtance: appearances, however, are viſibly in favour of the polar diameter's being the longeſt.

We find that the quick alterations in the viſible diſk of Mars, during the time it is in the beſt ſituation for us to obſerve it, are ſuch, that if we were to uſe many meaſures which have been taken of its diameters, we ſhould be obliged to have recourſe to a computation of its phaſes, in order to make proper allowance for them. Now, ſince theſe changes are in a longitudinal direction, and the poles of Mars are not perpendicular to the ecliptic, it would bring on a calculation of ſmall quantities, which it is always beſt not to run into where it can be avoided. For this reaſon, I ſhall at once ſettle the proportion of the equatorial to the polar diameter of this planet, from the meaſures which were taken on the very day of the oppoſition. I prefer them alſo on another account, which is, that they were made in a very fine, clear air, and were repeated with a very high power, and with two different inſtruments, of whoſe faithful repreſentation of celeſtial objects, the many obſervations on very cloſe double ſtars I have made with them have given me very evident proofs.

As we are at preſent only in queſt of the proportion of one diameter to the other, the meaſures of the 20-feet reflector, though not given in angular quantities, will equally ſuffice for the purpoſe. By them we have the equatorial diameter to the polar as 103 to 98, or as 1355 to 1289. I have turned the proportion into the latter numbers by way of comparing them the better with the meaſures of the 7-feet reflector. By that inſtrument the equator of Mars, Oct. 1. we find, was meaſured three times; but from the remarks annexed to the different reſults, I think the third meaſure ſhould be uſed. Indeed, on taking the difference of the two firſt, which is $34'''$, and dividing by three, we have the quotient $11\frac{1}{3}'''$; then, allotting two-thirds to the firſt, becauſe the remark ſays poſitively "narrow meaſure," it becomes $22''\ 34\frac{2}{3}'''$, and taking one-third from the ſecond, which is expreſſed doubtfully, "rather too full," it becomes $22''\ 35\frac{1}{3}'''$: this reflection on the two firſt meaſures gives additional validity to the third, which is $22''35'''$, or $1355'''$. The polar diameter was meaſured twice; and as no reaſon appears againſt either of the obſervations, I ſhall take the mean of both, which is $21''\ 29'''$, or $1289'''$; ſo that by theſe meaſures the equatorial diameter of Mars is to the polar as 1355 to 1289. A leſs perfect agreement between the proportions of the diameters ariſing from the meaſures of the 20-feet reflector and thoſe which we have juſt now deduced from the 7-feet, would have been ſufficient for our purpoſe, as we might eaſily have excuſed one or two thouſandths of the whole quantity; however, we have no cauſe to be diſpleaſed with this coincidence, though it ſhould in part be owing to accident, and therefore ſhall admit the above proportion, and proceed to a farther examination of it.

In the firſt place, it will be neceſſary to ſee whether any correction be required on account of the different heliocentric and geocentric ſouth latitude of Mars; which would apparently compreſs the polar diameter a little, by the defect of illumination on the north. On computation we find, that a difference ariſing from that cauſe would give the longitudinal diameter to the latitudinal as 20000 to 19987; which being much leſs than one thouſandth part of the whole, may therefore be neglected.

But next, a very conſiderable correction muſt be admitted, when we take into account the poſition of the axis of Mars. The declination of the ſun on that planet, at the time the meaſures were taken, was not leſs than 27° ſouth; ſo that the poles were not in the circumference of the diſk by all that quantity. On a ſuppoſition then, that the figure of Mars is an elliptical ſpheroid, we are now to find the real quantity of the polar diameter from the apparent one. It has been proved, that, in the ellipſis, the exceſſes of any diameters above the polar one are as the ſquares of the coſines of the latitudes*; but the diameter at rectangles to the equator of Mars, which was expoſed to our view in the late oppoſition, was not the polar one, but ſuch as muſt take place in a latitude of 63°. Putting therefore $m =$ coſine of 63°, $a = 1355$, $b = 1289$, $x =$ the polar axis, we have $1 : m^2 :: a - x : b - x$. And $\frac{b - m^2 a}{1 - m^2} = x$; which gives us 1272 nearly, for the polar diameter. The true proportion, therefore, of the equatorial to the polar diameter will be as 1355 to 1272; which, reduced to ſmaller but leſs accurate numbers, is 16 to 15 nearly.

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[*] Aſtr. par M. de la Lande, § 2680.

I ſhall now alſo mention ſome of the other meaſures, but with a view only to ſhew that they are very conſiſtent with the above determination. From thoſe of the 30th of September, for inſtance, we collect the proportion of the diameters of Mars as 1340 to 1286; or, reduced to our former numbers, 1355 to 1300. Now, ſince theſe meaſures were taken the night before the oppoſition, they muſt on that account be as good as the former; and, had thoſe of the day of oppoſition not been preferred, becauſe they were oftener repeated, and the ſuperior power of the 7, and great light of the 20-feet reflector, gave them additional weight, I ſhould have taken them into the account; the very ſmall difference, however, cannot but ſtrengthen the reſults of the former meaſures.

From the obſervations of the 25th of September we have the proportion of the diameters as 1313 to 1275; and if the equatorial meaſure be increaſed in the ratio of 20000 to 19953, on account of the different heliocentric and geocentric longitude, Mars not being at the full, it will give the ratio of 1316 to 1275; or, conforming to our former numbers, as 1355 to 1312. I have not been very ſtrict in the application of the correction deduced from the phaſes of Mars, ſince no other uſe was intended to be made of theſe numbers than merely to ſhew, that they do not very greatly differ from thoſe we have aſſigned before *.

It

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* If more ſtrictneſs be required, let EC, fig. 32. be the ecliptic; PS its poles; ps the poles of Mars, and eq its equator. Then, the angle pmC being found, by calculation, we ſhall have Cm (radius) to cm (coſine of the difference between the heliocentric and geocentric longitude) as qv (ſine of the angle qmv or pmC), to ov. Then, ſince with Mars Cc can never be very great, the ſmall triangle qno may be taken for ſimilar to qvm; therefore qm (radius) is to qv (ſine of pmC)

It was obſerved, Oct. 17, 1783, that the equatorial diameter of Mars was ſtill greater than the polar, notwithſtanding the depredation of the defect of light upon it. On calculating the phaſes, we find, that the longitudinal diameter was, that day, to the latitudinal one as 19711 to 20000, which therefore could not be an equal balance to oppoſe the ſpheroidical figure ſo as to render it inviſible.

But, Nov. 10. the proportion of the longitudinal diameter to the latitudinal one, from a computation of the phaſe of Mars, muſt have been as 18762 to 20000; and accordingly it was by obſervation found to be more than ſufficient to take off all appearance of the polar flattening, and leave a viſible exceſs in the axis above the equator.

To obviate any doubts concerning a fallacy that might ariſe from the convexity of the eye-glaſs, or irregular ſhape of the ſmall ſpeculum, I need only refer, for the latter, to the experiments of the 7th and 9th of October, 1783: for ſhould the ſhort diameter of my ſmall plane ſpeculum have occaſioned a compreſſing of the polar diameter of Mars when expoſed to it, half a turn of the teleſcope muſt bring the other diameter of that ſpeculum into the ſame ſituation, and a contrary effect would have followed. With regard to the former, not only the experiments made with the achromatic, but principally the obſervation with the 20-feet reflector, where I uſed a compound eye-piece magnifying only about 300 times, will ſufficiently exculpate the eye-glaſſes. It is alſo well known, that in a ſingle lens the diſtortion of the images, if any ſuch there

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pmO) as qo $(=qv-vo)$ to qn; which is the required correction or deficiency of the equatorial diameter eq of Mars.

Or, putting $mC=1$ and $vq=m=$ coſine of the angle Pmp; it will be $qn=m^2 \cdot cC$.

ſhould be, will equally affect the wires of the micrometer, and give a true meaſure notwithſtanding; and the compound eye-piece I uſed with the 20-feet reflector had likewiſe the ſame advantage, for it is conſtructed on the plan lately propoſed by Mr. Ramsden in the Philoſophical Tranſactions *, which he was ſo obliging as to communicate to me about a twelve-month ago, and which I immediately adapted to my large micrometers.

On the ſubject of the figure of Mars I ought to remark alſo, that perhaps the meaſures which were taken of its diameters during the laſt oppoſition will enable us to aſcertain its real ſize with greater accuracy than has been done before. The micrometer which can diſtinguiſh with preciſion between the equatorial and polar diameters of this ſmall planet, will certainly be admitted as an evidence of conſiderable conſequence; and ſince the reſult of theſe meaſures is pretty different from what former obſervations give us, I ſhould not omit mentioning it.

We have ſeen that the equatorial diameter, on the day of the oppoſition, meaſured 22″ 35‴. The diſtance of Mars from the earth at that time was .40457, the mean diſtance of the earth from the ſun being 1; therefore, 22″ 35″ reduced to the ſame diſtance will be no more than 9″ 8‴.

I ſhall conclude this ſubject with a conſideration relating to the atmoſphere of Mars. Dr. Smith † reports an obſervation of Cassini's, where "a ſtar in the water of Aquarius, at the "diſtance of ſix minutes from the diſk of Mars, became ſo "faint before its occultation, that it could not be ſeen by the "naked eye, nor with a 3-feet teleſcope." It is not mentioned

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* Vol. LXXIII. p. 94.

† Optics, § 1096.

tioned, what was the magnitude of the ſtar; but, from the circumſtance of its becoming inviſible to the naked eye, we may conclude, that it muſt have been of the ſixth or ſeventh magnitude at leaſt. The reſult of this obſervation would indicate an atmoſphere of ſuch an extraordinary extent, ſince at the diſtance of 36 ſemi-diameters of the planet it ſhould ſtill be denſe enough to render ſo conſiderable a ſtar inviſible, that it will certainly not be amiſs to give an obſervation or two which ſeem of a very different import.

1783, Oct. 26. There are two ſmall fixed ſtars preceding Mars, of different ſizes; with 460 they appear both duſky red, and are pretty unequal; with 278 they appear conſiderably unequal. The diſtance from Mars of the neareſt, which is alſo the largeſt, with 227 meaſured $3'\ 26''\ 20'''$. Some time after, the ſame evening, the diſtance was $3'\ 8''\ 55'''$, Mars being retrograde. I ſaw them both very diſtinctly. I viewed the two ſtars with a new 20-feet reflector of 18,7 inches aperture, and found them, as I expected, very bright.

Oct. 27. I ſee the two ſmall ſtars again. The ſmall one is not quite ſo bright in proportion to the large one as it was laſt night, being a good deal nearer to Mars, which is now on the ſide of the ſmall ſtar; but when I draw the planet aſide, or out of view, I ſee it then as well as I did laſt night. The diſtance of the ſmall ſtar meaſured $2'\ 56''\ 25'''$ *.

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* The meaſures were accurate enough for the purpoſe, though not otherwiſe to be depended on nearer than, perhaps, ſix or eight ſeconds.

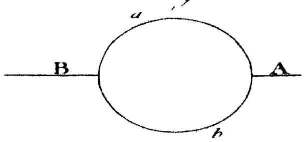
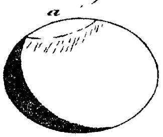


Fig 1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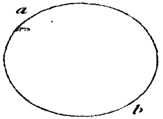
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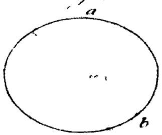
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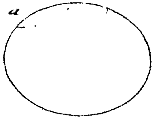
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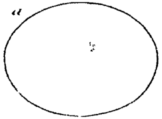
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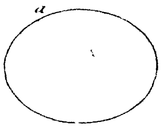
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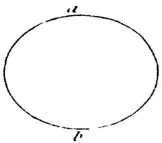
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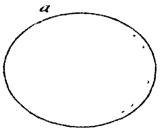
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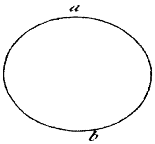
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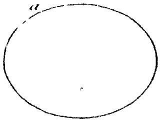
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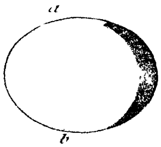
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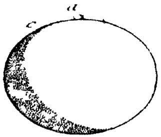
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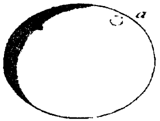
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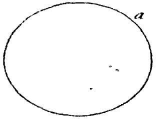
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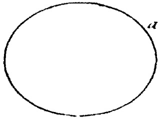
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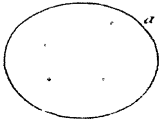
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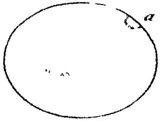
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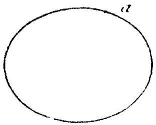
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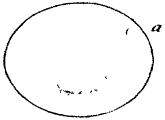
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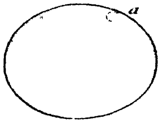
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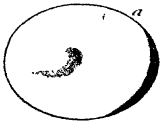
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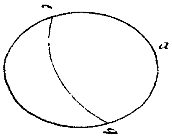
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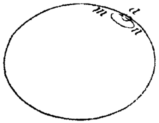
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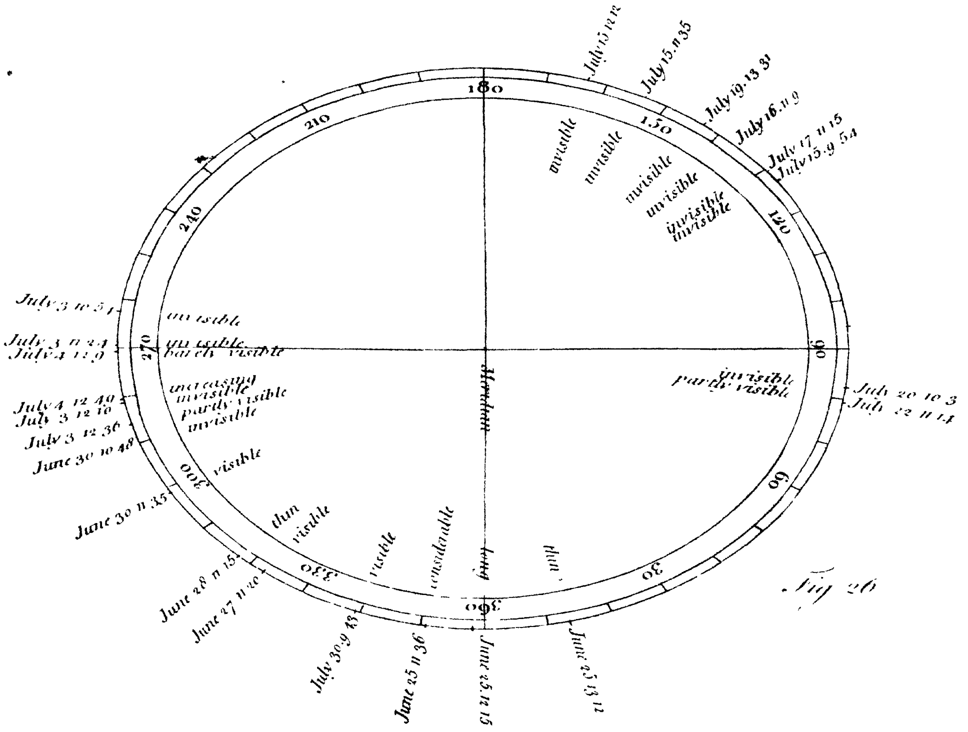
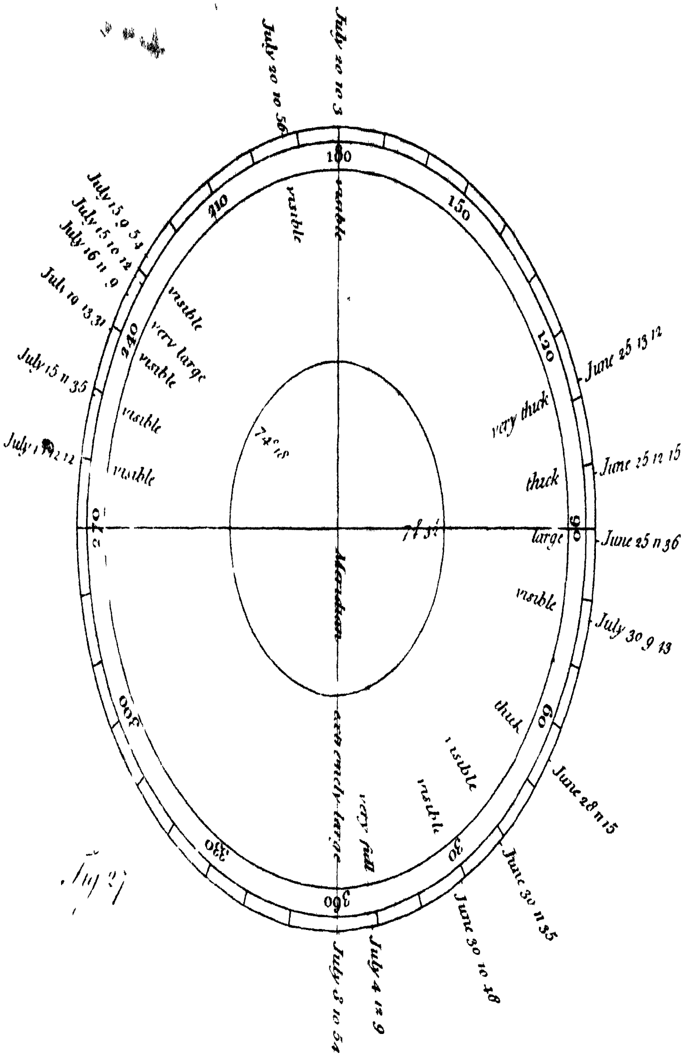


Fig. 26.

Track of the bright north polar spot on Mars. in June & July 1781.



Track of the bright south polar spot on Mars.
in June & July 1781.

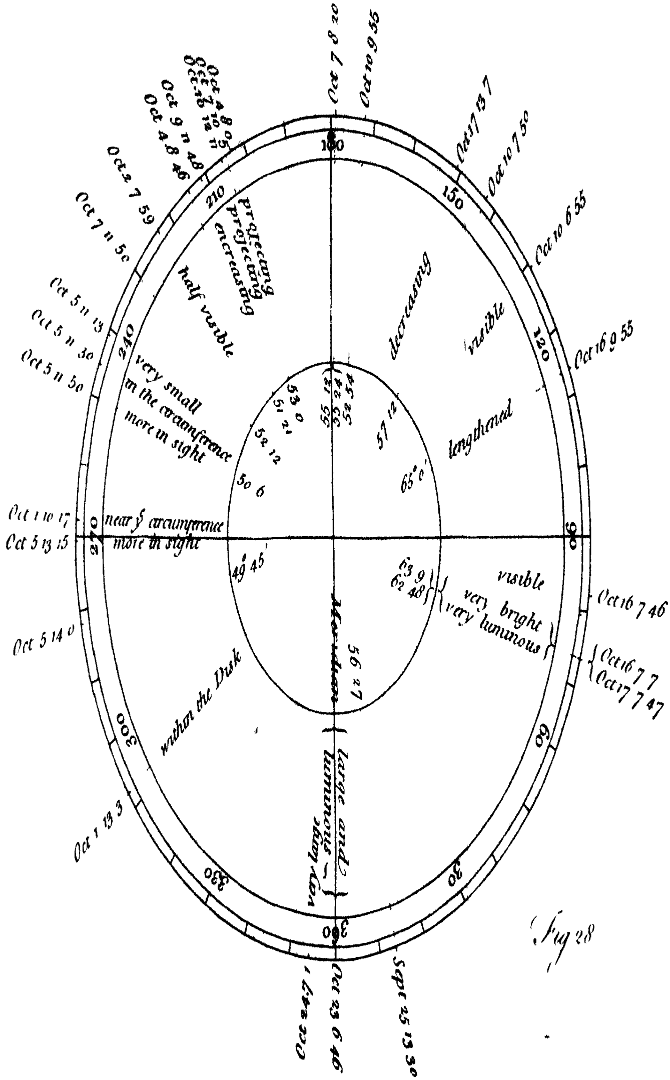
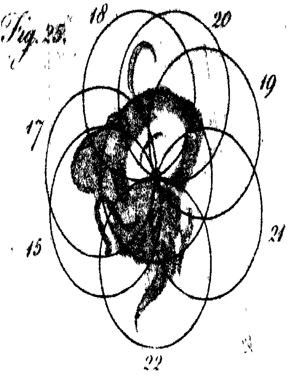


Fig 28

Track of the bright South polar spot on Mars, in October 1783.

Fig. 28.



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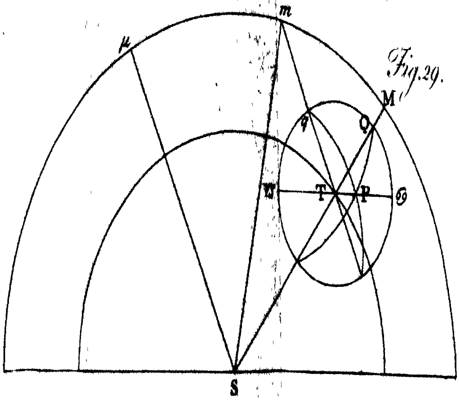


Fig. 30.

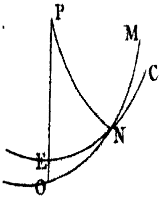


Fig. 31.

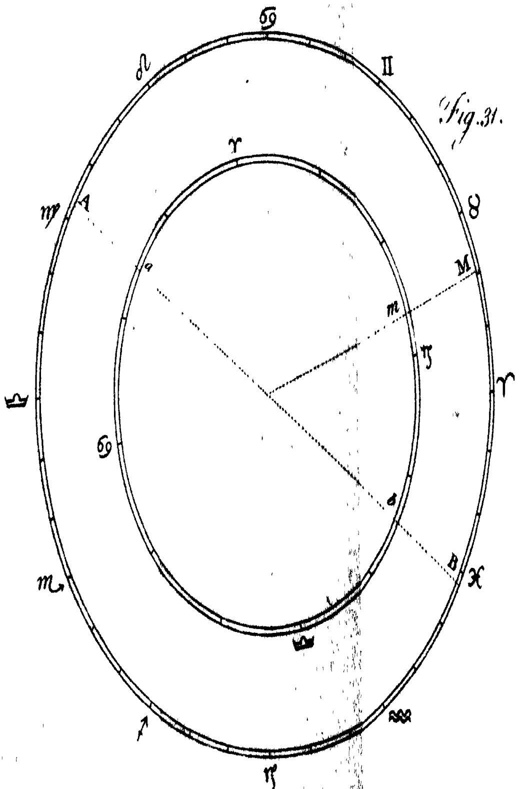
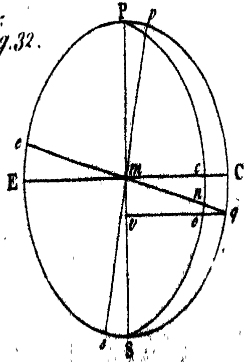


Fig. 32.



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The largeſt of the two ſtars on which the above obſervations were made cannot exceed the twelfth, and the ſmalleſt the thirteenth or fourteenth magnitude; and I have no reaſon to ſuppoſe that they were any otherwiſe affected by the approach of Mars, than what the brightneſs of its ſuperior light may account for. From other phænomena it appears, however, that this planet is not without a conſiderable atmoſphere; for, beſides the permanent ſpots on its ſurface, I have often noticed occaſional changes of partial bright belts, as in fig. 1. and 14.; and alſo once a darkiſh one, in a pretty high latitude, as in fig. 18. And theſe alterations we can hardly aſcribe to any other cauſe than the variable diſpoſition of clouds and vapours floating in the atmoſphere of that planet.

Reſult of the contents of this paper.

The axis of Mars is inclined to the ecliptic 59° 42′.

The node of the axis is in 17° 47′ of Piſces.

The obliquity of the ecliptic on the globe of Mars is 28° 42′.

The point Aries on the martial ecliptic anſwers to our 19° 28′ of Sagittarius.

The figure of Mars is that of an oblate ſpheroid, whoſe equatorial diameter is to the polar one as 1355 to 1272, or as 16 to 15 nearly.

The equatorial diameter of Mars, reduced to the mean diſtance of the earth from the ſun, is 9″ 8‴.

And that planet has a conſiderable but moderate atmoſphere, ſo that its inhabitants probably enjoy a ſituation in many reſpects ſimilar to ours.

Datchet, Dec. 1, 1783. W. HERSCHEL.

XX. *A Deſcription of the Teeth of the* Anarrhichas Lupus Linnæi, *and of thoſe of the* Chætodon nigricans *of the ſame Author; to which is added, an Attempt to prove that the Teeth of cartilaginous Fiſhes are perpetually renewed. By Mr.* William Andre, *Surgeon; communicated by Sir* Joſeph Banks, *Bart. P. R. S.*

Read March 18, 1784.

THE amazing variety there is in the external form of fiſhes muſt be obvious to a common obſerver; and whoever examines will be convinced, that the ſame variety prevails in their internal ſtructure. No parts, perhaps, afford a more convincing proof of the laſt aſſertion than the teeth of fiſhes. To adduce a few inſtances, let us only recollect the tuberculated teeth in the thorn-back; the triangular ſerrated teeth in the ſhark; the ſlender flexible teeth in the *chætodontes*, or angel-fiſhes. There is not only a difference of their form, but alſo in the ſubſtances of which they are compoſed; ſome being of a ſoft horny nature; others made up of bone; others of that ſubſtance we call enamel in the teeth of quadrupeds; and ſome having the apparent hardneſs and tranſparency of cryſtal. We may alſo notice their uncommon ſituation; many fiſhes having teeth not only in their jaws, but on the tongue, the palate, and about the *fauces*.

To illuſtrate in ſome degree this part of natural hiſtory, I ſhall deſcribe the teeth of the *Anarrhichas Lupus*, or Sea-wolf, and thoſe of the *Chætodon nigricans*, a ſpecies of

Angel-fiſh. The former have been but imperfectly deſcribed, and never repreſented diſtinct from the fiſh, without which it is impoſſible to have any exact idea of their diſpoſition, number, or form, while the true ſhape and compoſition of the latter, from their minuteneſs, have been entirely overlooked. I ſhall then attempt to prove, that a continual renovation of the teeth takes place in cartilaginous fiſhes.

The Sea-wolf is a fierce and ravenous fiſh, as its name imports, found in the northern parts of the globe, where it frequently grows to the length of four feet and upwards.

The jaws of the Wolf-fiſh are made up of ſeveral bones, to each of which a greater or leſs number of teeth are affixed; but, before I enter upon the deſcription of them, I ſhall take notice of the palate (marked A. tab. XI.), that being a kind of baſis or ſupport to the other bones, to which they are all more or leſs connected. The palate is a thick and firm bone united above to the bones of the *cranium* and noſe, and ending below in a flat oval ſurface, on which are incruſted about twelve or thirteen ſtrong, blunt, and rather flat teeth of the *molar* or grinder kind. The external edges of the teeth are the moſt prominent; by which means a hollow is formed in the middle of the palate.

The upper jaw is compoſed of three bones, two of which (BB) are placed laterally, forming the ſides of the upper jaw, and the third (C) anteriorly, making the fore-part of the jaw. The third bone may be divided through its middle into two portions; but ſince it has the appearance of one bone only, the connection being very firm, I ſhall deſcribe it agreeably to that appearance, to prevent needleſs diviſions.

The ſide bones of the upper jaw have nearly the ſhape of an italic *ſ*. At their poſterior ends may be obſerved a ſmooth articular ſurface, for their connection with a ſimilar ſurface on the poſterior extremities of the lower jaw; and on their anterior ends there are two rows of teeth. The external row conſiſts of three or four ſharp or conical teeth; and the internal row of four or five blunt and rather flat ones. Theſe bones are connected to the palate and bones of the noſe by looſe but ſtrong ligaments.

The third bone of the upper jaw, which may be called the anterior or naſal portion, is of a triangular form, connected above to the bones of the noſe, and ending below in a flat ſurface, thick-ſet with ſharp conical teeth. The external teeth, about four in number, are large and ſtrong, and bend a little inwards; but the internal ones are ſmall, and nearly ſtraight, of which we may reckon about ten.

This bone is connected above (as I have before obſerved) to the bones of the noſe; between which a complete joint is formed, of that kind called by anatomiſts *ginglymus*, that is, where the projecting parts of one bone are received by correſponding cavities in the other. Like other articulations, it is furniſhed with a capſular ligament, and no doubt an apparatus for the ſecretion of *ſynovia*. Although a joint exiſts between this bone, and thoſe of the noſe, yet no muſcles are provided for its motion, which depends entirely upon the reſiſtance made by thoſe hard bodies which the animal takes into its mouth.

The lower jaw (D) conſiſts of two bones, united at their fore-parts by a ſtrong ligament, which allows of ſome motion. On their anterior extremities are placed ſix large and as many ſmall ſharp and conical teeth; the large teeth are placed externally, and their points are bent a little inwards; while the ſmall ones, which ſtand within them, are nearly ſtraight. Behind theſe are two or three rows of grinder teeth. The external teeth ſtand nearly upright; but the internal ones are placed obliquely, inclining towards each other.

The teeth are formed of a hard bony matter, not covered with enamel as in ſome animals; nor is there an equal diſtribution of enamel and bone as in ſome others. They are not fixed in ſockets, but are faſtened to the jaws in the ſame manner as the *epiphyſes* are united to the bodies of the bones in young animals.

From the foregoing deſcription it will appear, that the anterior ſharp teeth of the Sea-wolf are admirably calculated for ſeizing its prey, while the poſterior grinding teeth ſerve to break down the hard ſhells of lobſters, crabs, muſcles, ſcollops, &c. which this animal is known to feed upon. The external teeth on the ſides of the upper and lower jaw being higher than thoſe placed within them, a hollow is formed above and below, in which the convex ſhells of cruſtaceous animals, &c. are confined during their compreſſion between the jaws, which is effected by the action of ſtrong muſcles placed on the ſides of the head. The jaws being made up of a number of pieces, and connected by looſe ligaments, a freedom of motion is allowed, and the colliſion or ſhock ariſing from the comminution of hard bodies is ſo much the leſs by being divided among a number of bones.

Merret informs us [*], the *lapis buſonites* are the flat grinder teeth of this fiſh petrified. But certainly theſe foſſils are not the production of the Sea-wolf alone, ſince they may originate from all thoſe fiſhes which have flat teeth in their palate or jaws; a ſtructure which the French naturaliſts diſtinguiſh by the appellation of *palais pavé*.

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[*] Pinax Rerum Naturalium Britannicarum.

OF THE CHÆTODON NIGRICANS.

The individual which furniſhed the following account was brought from the Weſt Indies, and meaſured about five inches in length *. Its teeth (the only parts I mean to deſcribe) were ſo ſmall as to require the aſſiſtance of a microſcope to diſcover their real ſhape. There were fourteen teeth in each jaw, ſeven of which from the upper one are repreſented tab. XII. They conſiſt of a cylindrical body fixed in the jaw, above which they ſpread out into a broad and rather flat ſurface, on the edges of which are twelve or thirteen *denticuli*, making an uncommon appearance, and totally different from the teeth of any other animal. Another ſingularity is their being tranſparent, unleſs viewed with a deep magnifier, when a few opaque lines may be perceived, which point out the cellular part of the tooth through which the blood veſſels ramify, which are deſtined for its growth and nouriſhment. They are not all of the ſame length. Thoſe in the anterior parts of the jaws are the longeſt, from whence they gradually diminiſh in length as they approach the angles of the mouth.

From the foregoing deſcription of the teeth of the *Chætodon nigricans*, this fiſh ſeems to be miſplaced in the *Syſtema Naturæ* of LINNÆUS; ſince one generic diſtinction of the *Chætodontes* is to have numerous, ſlender, and flexible teeth; whereas the teeth of the *Chætodon nigricans* are few in number, placed in one row, and of a cryſtalline hardneſs.

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* This fiſh is well repreſented in DU HAMEL Traité général des Pêsches, tom. III. ſeconde partie, ſection IV. planche xii. under the name of *Chirurgien ou Porte Lancette*.

OF THE TEETH OF CARTILAGINOUS FISHES.

When STENO examined the teeth of the ſhark, he was ſurpriſed to find a great number of them placed on the inſide of each jaw, lying cloſe to the bone, and many of them buried in a looſe ſpongy fleſh; concluding that theſe internal teeth could be of little or no uſe to the animal. Mr. HERISSANT * afterwards ſhewed the uſe of theſe internal or poſterior teeth, by proving, that as the anterior teeth of each row are broken off, drop out, or wear away, the poſterior ones come forward to ſupply their places †.

But though it be certain that the anterior teeth, when loſt, are replaced by the poſterior ones, neither of the above naturaliſts, or any other that I know of, have attempted to aſcertain how often this circumſtance happens. Whether the renovation be perpetual during life; or whether that operation be ſuſpended after a limited number of teeth have been ſupplied.

From a ſingular circumſtance, which I met with ſome time ago, I am inclined to think the former is the fact; or, that in cartilaginous fiſhes, ſuch as ſharks, rays, &c. there is a perpetual renovation of the teeth.

Being engaged in diſſecting the jaws of a very large ſhark, I was ſurpriſed to find a portion of that ſharp, bearded bone found in the tail of the fire-flaire, or ſting-ray ‡, driven quite

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* BOMARE Dictionnaire d'Hiſtoire Naturelle, article Requien.

† It may not be improper on this occaſion to point out a miſtake which ſome naturaliſts have fallen into, in allowing a ſet of muſcles for raiſing the numerous teeth placed in the jaws of ſharks. I have frequently diſſected the jaws of thoſe animals, and am certain no ſuch muſcles exiſt, nor are they indeed at all neceſſary.

‡ *Raia Paſtinaca* LINNÆI. The French naturaliſts, on account of the bone in the tail, call this fiſh [illegible]

through the lower jaw among the poſterior teeth, and fixed almoſt immoveably. How this happened muſt be obvious to every one. (See the figure, tab. XIII.)

Before I proceed, it will be neceſſary to obſerve, firſt, that the poſterior teeth of cartilaginous fiſhes are always found in a ſoft, membranous ſtate, and but imperfectly formed; notwithſtanding this, they have the whiteneſs of teeth from a ſmall quantity of calcareous earth already depoſited within their ſubſtance. Their hardneſs and perfect form is acquired as they advance towards the anterior parts of the jaws. Secondly, that of the three angles in each tooth of the ſhark, one is placed towards the right, another towards the left, and the other, which is in the middle, and the moſt acute angle, is directed inwardly towards the tongue or *fauces*. They are placed then in ſuch a manner as that the angles of the teeth on the left-ſide in one row, approach the angles of the teeth on the right-ſide in the next row. Thoſe teeth which ſtand on a line from without inwards, I call a row; not thoſe which are placed nearly in a parallel line from one ſide of the mouth to the other.

The ſharp bone of the ſting-ray was fixed in the lower jaw between two rows of teeth, and at their poſterior part, where the firſt rudiments of the future teeth are formed, and it will be clear to every one, particularly thoſe who are converſant in ſuch matters, that this could not have happened without producing a great deal of pain, ſwelling, and diſorder in the part where it was fixed. It is unneceſſary to enumerate the different kinds of miſchief this might occaſion. Let it ſuffice to obſerve, that on account of the ſpace taken up by this extraneous body, the teeth on each ſide of it, for want of room, could never after be perfectly formed. The teeth on the left-ſide wanting their angles to the right, and the teeth on the right-ſide being deſtitute of their angles to the left.

As it is certain, that the anterior teeth were formerly poſterior ones; and as the teeth in each row were all deficient in one angle, it follows, that they muſt have been formed poſterior to the inſertion of this extraneous body. Again, if we allow that before the accident the animal was in poſſeſſion of perfect teeth, it follows alſo, that they were conſumed and replaced by imperfect ones.

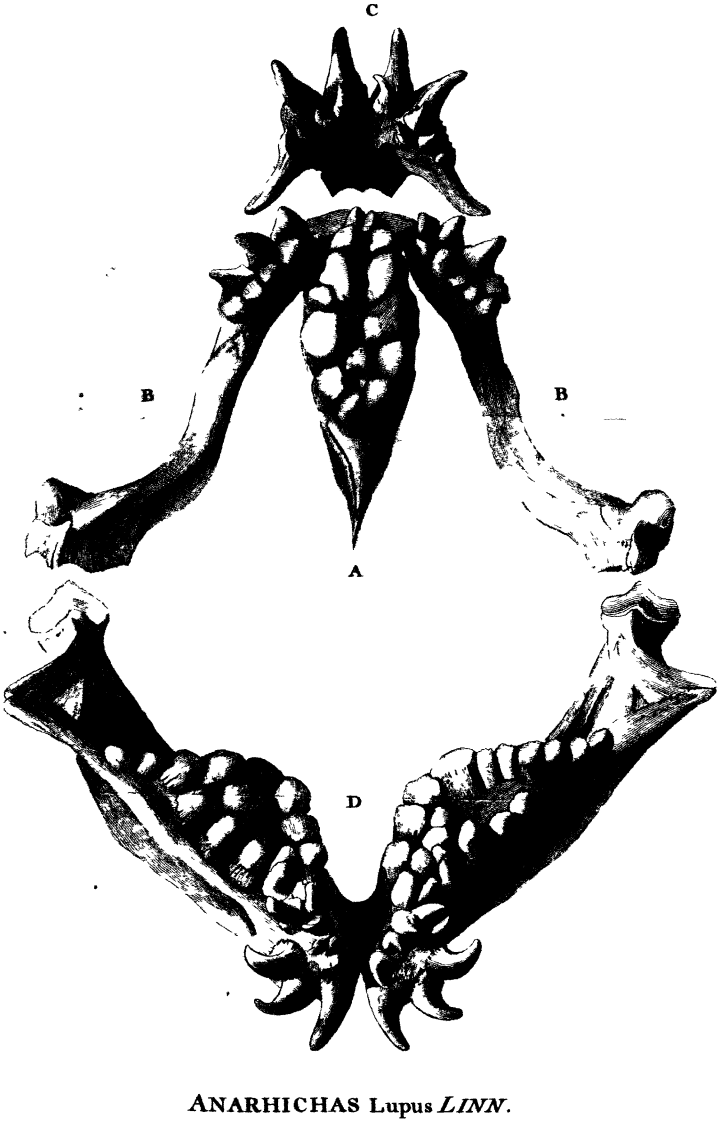
There were ſix teeth in each row, and fifty-two rows, making together about 312 teeth. Now allowing the conſumption to have been equal in all parts of the jaws, it follows, that the animal had already conſumed 312 teeth, and was in poſſeſſion of a like number for future conſumption.

The teeth of ſharks, rays, &c. may be divided into active and paſſive. The active teeth are the anterior ones of each row, ſtanding with their points upwards. The paſſive teeth are the remaining ones, lying one upon another, like the tiles upon a houſe (imbricated), with their points downwards. It appears from the foregoing account, that the anterior or active teeth had been replaced ſix times; and that they might have been renewed ſix times more, making in all twelve times. From which, I think, we may reaſonably conclude, that this does not happen any preciſe number of times; but that the renovation is perpetual during the life of the animal.

The longevity of fiſhes is a fact pretty well eſtabliſhed. In addition to this part of natural knowledge, I have endeavoured to prove, that a part of the inhabitants of the great deep retain, in the article of teeth, a perpetual juvenility, being apparently utter ſtrangers to edentulous old age.

EXPLANATION OF THE PLATES.

Tab. XI. The jaws of the Wolf-fiſh.
A. The palate.
BB. The ſide bones of the upper jaw.
C. The anterior or naſal portion of the ſame.
D. The lower jaw.
Tab. XII. The teeth of the *Chætodon nigricans* magnified.
Tab. XIII. Part of the lower jaw of a large ſhark.
AA. Two rows of perfect teeth.
BB. Two rows of imperfect ones.
C. The bone of the ſting-ray.



ANARHICHAS Lupus *LINN.*

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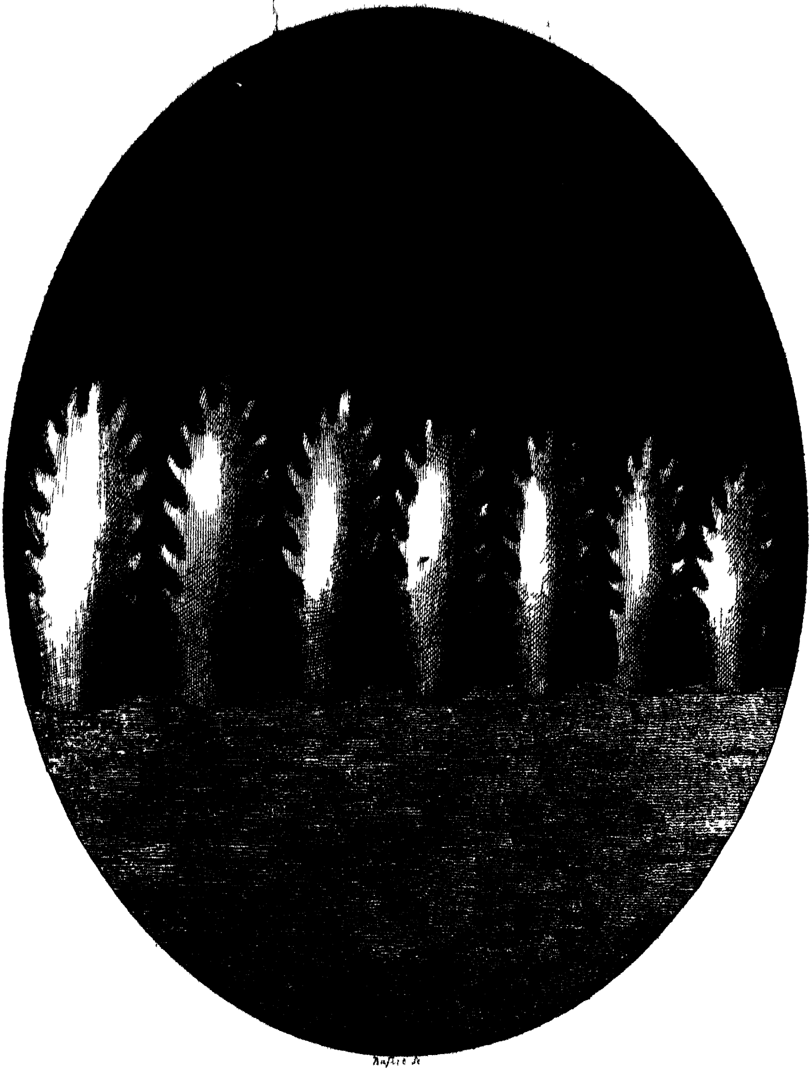

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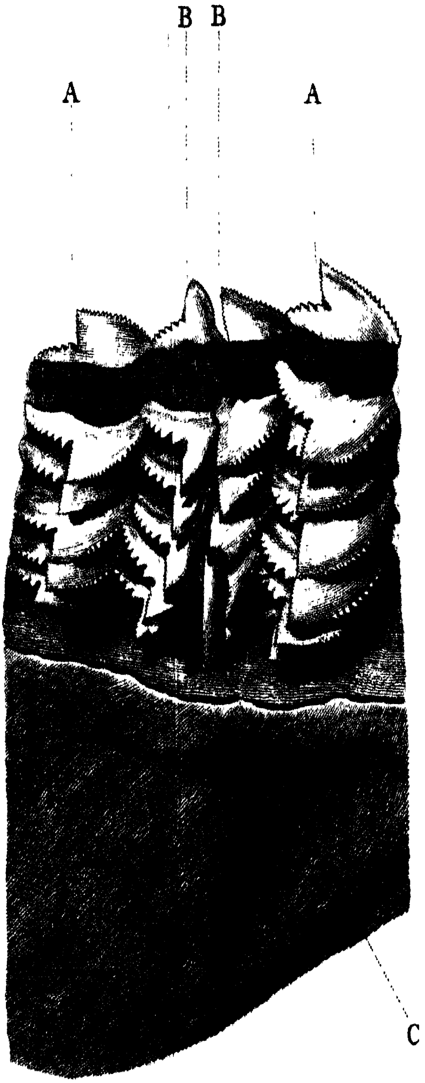
CHÆTODON nigricans *LINN.*


B
B
A
A
C

XXI. *Abſtract of a Regiſter of the Barometer, Thermometer, and Rain, at* Lyndon, *in* Rutland, 1783. *By* Thomas Barker, *Eſq.*; *communicated by* Thomas White, *Eſq. F. R. S.*

Read March 18, 1784.

		Barometer.			Thermometer.						Rain.		
					In the Houſe.			Abroad.			Lyndon	Sel-bourn, Hampſh.	South Lambeth, Surrey.
		Higheſt	Loweſt	Mean.	High.	Low.	Mean	High.	Low.	Mean			
		Inches.	Inches.	Inches.	°	°	°	°	°	°	Inch.	Inch.	Inch.
Jan.	Morn.	29,87	28,38	29,04	47	33½	41	46½	18½	35	1,805	4,43	1,51
	Aftern.				48	35	42	50	27½	40			
Feb.	Morn.	30,12	28,08	29,28	48	36	43	49	17½	36½	2,313	5,54	2,98
	Aftern				49½	37	43½	53½	30	43			
Mar.	Morn.	30,01	27,88	29,28	47½	33½	40	44½	21½	33	1,604	2,16	,93
	Aftern.				49½	33½	41½	55	33	43			
Apr.	Morn.	30,14	29,15	29,70	58	46	51	52	32½	43	0,558	,88	,59
	Aftern.				62½	47½	53	67½	41½	55½			
May	Morn.	29,82	29,13	29,48	63½	47	53	55½	34½	44	4,218	2,84	2,36
	Aftern.				66	48½	55	74½	44	56			
June	Morn.	29,85	28,80	29,47	65½	55½	60	63½	46	55	3,033	2,82	4,00
	Aftern.				68½	56½	61½	79	55	67			
July	Morn.	29,89	29,16	29,55	72	61½	66	68	55	61½	2,663	1,45	,78
	Aftern.				75	67	68½	85	67	74			
Aug	Morn.	29,83	29,17	29,49	67	57½	62	67½	46	57	1,102	2.24	2,23
	Aftern.				75	58½	64½	84	57	67			
Sept.	Morn.	29,87	28,77	29,36	61½	54	57½	59	40	50½	1,440	5,53	4,30
	Aftern.				64	56	59	68	55	60			
Oct.	Morn.	29,88	28,99	29,48	60½	46½	53	58	34	44½	0,658	1,71	,72
	Aftern.				61½	48	54	64½	44	54			
Nov.	Morn.	29,96	28,42	29,45	51	42	46½	52½	30½	40	1,783	3,01	1,63
	Aftern.				52	42½	47	54½	37½	45½			
Dec.	Morn.	29,99	28,49	29,29	46	28	40½	43½	8½	32½	1,602	1,10	1,22
	Aftern.				46½	28	40	45½	19	37			
										Inches	22,779	33,71	23,25

The year began with a ſhort dry froſt, then ſhowery, intermixed with froſt. The end of January, and near half of February, ſtormy and wet, and after ten days fine and mild; a ſevere ſeaſon for ſnow, wet, wind, and froſt. The end of February and beginning of March cut the graſs, corn, and ſtock, more than all the winter before. From March 10. to May 27. was a very dry ſeaſon and fine ſeed-time; but ſo dry at laſt the late ſown corn could not come up. The ſpring was pleaſant; but almoſt conſtant froſty mornings till April, and frequent afterward, kept things backward; and though there was ſome fine warm weather the middle of April, yet later in May the drought and N.E. winds ſtopped the growth of things; and two ſharp froſty nights, May 25. and 26. the rime was ſo particularly cutting in the meadows, that the young ſhoots on many oak and aſh trees in the vallies were entirely killed, while thoſe on the hills were unhurt, and ſome of the tops of the trees eſcaped, though the bottoms were blaſted.

May 27. to 30. in a continued three days rain there fell 3¾ inches, which is, I believe, the moſt that has come in one continued unceaſing rain ſince July 1736, when, in about the ſame time, there came five inches; but the rain this May was not alike in all places, for there was not a quarter ſo much in Hampſhire. This rain was of vaſt ſervice to bring up the late ſown corn, and make the graſs grow well; but this and ſome other haſty rains afterwards hurt the meadow graſs, by flooding it three times. Hot weather ſucceeding, it was a very growing time, and ten days together, in the middle of June, were all wet.

During the ſhowery time an uncommon hazineſs began, which was very remarkable all the reſt of the ſummer: the air was all thick both below the clouds and above them, the hills looked blue, and at a diſtance could not be ſeen; the ſun ſhone very red through the haze, and ſometimes could not be ſeen when near ſetting. There was more or leſs of this haze almoſt conſtantly for a month, and very frequently to the end of the ſummer, and it did not ceaſe till Michaelmas; and neither rain nor fair, wind nor calm, eaſt nor weſt winds, took it away; and it was as extenſive as common, for it was the ſame all over Europe, and even to the top of the Alps. This haze was very like VIRGIL's deſcription of the ſummer after J. CÆSAR's death, which was probably the ſame caſe,

Cum caput obſcurâ nitidum ferrugine texit,

for ruſty iron is a very good deſcription of the colour the ſun ſhone. But by PLUTARCH's account, near the end of C. CÆSAR, that ſummer was very different from this in other reſpects for, he ſays, the ſun gave very little heat, the air was cloudy and heavy, and the fruits not ripened, which was not the caſe this year; for this was a dry haze, the ſummer in general hot and dry, and in ſome countries very much ſo.

I think I never knew more miſchief done by thunder than there was in different places this year, from the beginning of July, and very ſeldom more or hotter weather; yet where they had not thoſe thunder-ſhowers they ſuffered by being burnt up. Here we never wanted graſs after May, and the hay and harveſt were both well got in; but in Surrey, Hampſhire, and Dorſet, they were very much burnt up, had little hay, and as they had a good deal of ſhowery weather in harveſt, their barley ſuffered twice, from not coming up in time, and again in getting it in. As the rain this year was chiefly in ſhowers or ſudden rains, it fell very uncertainly, as appears by comparing what fell here with that in Hampſhire. The latter part of Auguſt and firſt half of September was ſhowery; but in this country not ſo much as to hurt the harveſt, a great part of which was in firſt. The crop of grain was in general pretty good, but did not yield enough to make up the defect of the laſt year's crop, every body was ſo much out of all ſorts, as the corn laſt year was both ſcarce and bad; grain, therefore, continued dear this year, eſpecially barley.

The ſummer 1782 had been ſo cold and wet, that the flower buds on many trees were very ſmall and not perfected, ſo that this ſpring there was a great want of bloſſoms on the wall fruit and apples, and exceeding few indeed on aſh-trees and hawthorn. I do not know of any aſhkeys at all, nor any bunches of haws, only a few ſcattered ſingle ones; but cherries and plumbs bloſſomed well, and there was no want of fruit; plenty of currants, and vaſt quantities of gooſeberries.

Auguſt 18. a remarkable ball of fire was ſeen between nine and ten at night all over England, and even in foreign countries. It ſeemed to move from north to ſouth or ſouth-eaſt. There was another October 4. but not ſo much obſerved, and ſome ſay another afterward, but little ſeen; but there were very few northern lights this autumn.

The autumn was a very fine one; calm, fair, and mild, but rather too dry for the ſowing of wheat, which, however, in general came up well, and what lay dry was brought up very finely by ten days wet the middle of November; after which it was dry and fine again, an open mild time, with few froſty mornings; but a good deal of dark or miſty weather in December, yet mild till the laſt week, when there came a great ſnow, very ſevere froſt, and cutting ſtrong wind, which ended the year.

XXII. *On the Periods of the Changes of Light in the Star* Algol. *In a Letter from* John Goodricke, *Eſq. to the Rev.* Anthony Shepherd, *D. D. F. R. S. Profeſſor of Aſtronomy at* Cambridge.

Read April 1, 1784.

SIR, York, Dec. 8, 1783.

AS I am now able, by collating ſome of my late obſervations on Algol with thoſe I ſent you laſt May, to determine with greater preciſion the periodical return of its changes, I wiſh to add this as a kind of ſupplement to that account.

The method I have here purſued is by taking the intervals between accurate obſervations of Algol's leaſt brightneſs or greateſt diminution of light made at long diſtances of time from each other, and dividing thoſe intervals by a certain number of revolutions, as will be beſt underſtood by the table below. The reaſon of my chuſing long intervals is, that the number of revolutions being greater, the errors of obſervation are thereby diminiſhed: all error cannot, however, as yet be excluded, but I think the period is now, by the following calculation, aſcertained within ten or fifteen ſeconds.

Mean times of Algol's
least brightneſs.

1783	h.	′				d.	h.	′	″
Jan. 14 Oct. 25	9 6	25 39	an interval of	99	revolutions, each of	2	20	49	14
Jan. 14 Nov. 14	9 8	25 17	Ditto	106	Ditto	2	20	49	10
Jan. 14 Nov. 17	9 4	25 52	Ditto	107	Ditto	2	20	49	2
Feb. 6 Oct. 25	8 6	15 39	Ditto	91	Ditto	2	20	49	3
Feb. 6 Nov. 14	8 8	15 17	Ditto	98	Ditto	2	20	48	59
Feb. 6 Nov. 17	8 4	15 52	Ditto	99	Ditto	2	20	48	51
Feb. 26 Oct. 25	9 6	43 39	Ditto	84	Ditto	2	20	49	14
Feb. 26 Nov. 14	9 8	43 17	Ditto	91	Ditto	2	20	49	9
Feb. 26 Nov. 17	9 4	43 52	Ditto	92	Ditto	2	20	49	0
Jan. 31 Nov. 14	14 8	29 17	Ditto	100	Ditto	2	20	49	4
Mar. 21 Nov. 17	8 4	36 52	Ditto	84	Ditto	2	20	48	46
			Hence the period of Algol's variation is, on a mean,			2	20	49	3

I could have added ſeveral more compariſons of the like kind; but theſe are, I think, ſufficient. It is to be remembered, that all the obſervations contained in the above table are reduced to mean time.

It appears to me now, that the duration of the variation is about eight hours; but, as it is difficult to hit exactly the beginning and end of the variation, this may occaſion different obſervers to differ in this reſpect. Before I conclude, I beg leave to mention a circumſtance deſerving of notice; which is, that FLAMSTEAD has alſo amongſt other ſtars obſerved Algol, and in two places has marked it of leſs magnitude than at other times, *viz.* of the third magnitude, 1696, January 16. 6 h. 24′, and 1711, December 5. 9 h. 13′, both mean time and old ſtile *. Suſpecting theſe might probably be days of Algol's variation, I computed the interval between them, but could not find a period anſwerable to that which I have above determined. Upon examining more cloſely the obſervations, I find, in that of 1696, he marked at the ſame time the magnitude of ρ Perſei; which, conſidering eſpecially the nearneſs of ρ Perſei to Algol, makes this obſervation to be relied on for its juſtneſs, and leſs liable to any miſtake of judgement; whereas the other obſervation of December 5, 1711, is more liable to error or doubtfulneſs, becauſe he did not then mark the magnitude of ρ Perſei, or of any ſtar of the ſame magnitude near enough to Algol. Preſuming, therefore, on the juſtneſs of FLAMSTEAD's obſervation of 1696, to think that it probably was made at a time when Algol varied, I compared it with one of mine, *viz.* October 25. 6 h. 39′, 1783, and I find there is, in the interval between thoſe obſervations, either 11,176 periods, each of 2 d. 20 h. 49′ 18″; or 11,177, each of 2 d. 20 h. 48′ 56″. The laſt, as it approaches neareſt to the reſults of my beſt obſervations, I think, is the exacteſt determination of the period. This, however, all proceeds upon the ſuppoſition that Algol varied at the time of FLAMSTEAD's obſervation, and alſo that the period is regular.

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* Hiſtoria Cœleſtis, vol. II. edit 1725, p. 284. and 534.

The following is a ſhort abſtract of my late obſervations on Algol, when its leaſt magnitude was accurately determined.

August 17, 1783.

App. time.

h. ′

10 52 About equal to ρ Perſei, though Algol ſeemed to be rather brighter.

11 7 Evidently leſs than ρ Perſei.

11 22 Ditto; but rather difficult to diſtinguiſh them from each other.

11 30 Rather brighter than ρ, and not ſo bright as δ Perſei.

12 0 About the brightneſs of δ Perſei, and rather leſs than β Trianguli.

12 30 Brighter than δ Perſei, and rather not ſo bright as β Trianguli.

From thoſe obſervations, by taking a mean between 11 h. 7′ and 11 h. 22′, it appears, that its leaſt brightneſs happened at 11 h. 14′; true, I think, to 5′.

October 25.

6 40 It was conſiderably leſs than ρ Perſei.

7 5 Ditto.

7 20 Equal to ρ Perſei, though Algol ſeemed rather leſs.

7 35 About equal to ρ Perſei.

7 50 Brighter than ρ, and alſo than δ Perſei.

8 25 About the third magnitude, and equal to β Trianguli.

9 35 Between the ſecond and third magnitude; brighter than β Arietis, and rather leſs than α Pegaſi.

10 10 About the ſecond magnitude; rather brighter than α Pegaſi, rather leſs than β Caſſiopeæ, and not ſo bright as α and γ Caſſiopeæ.

Rather

App. time.
h. ′

10 40 Rather brighter than β Caſſiopeæ, but leſs than α and γ.

11 0 Nearly equal to, if not rather brighter than, γ Caſſiopeæ, and leſs than α Caſſiopeæ*.

In 20′ afterwards it was of the ſame brightneſs; hence we may conclude, that the variation has ended at 11 h. 0′.

Its leaſt brightneſs from the obſervations appears to have happened at 6 h. 55′; true, I think, to 10′.

November 11.

10 5 Third magnitude; not much different from ζ Perſei and β Trianguli.

10 45 Between the third and fourth magnitude; believe equal to δ Perſei.

11 14 Leſs than ρ Perſei.

11 48 Ditto; but think it rather increaſed.

Its leaſt brightneſs from thoſe obſervations appears to have happened at 11 h. 31′; true, I believe, to a quarter of an hour. The weather was rather hazy.

November 14.

5 0 Between the ſecond and third magnitude, and leſs than β Caſſiopeæ.

5 45 A little brighter than β Arietis.

6 50 Not ſo bright as β Arietis, and rather brighter than β Trianguli.

8 10 A little brighter than ε Perſei, and believe equal to δ Perſei.

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* Algol's uſual and greateſt brightneſs, by my later and more accurate obſervations, is thus: a little leſs than α Caſſiopeæ, brighter than β Caſſiopeæ and α Pegaſi, and rather a little brighter than γ Caſſiopeæ.

Leſs

App. time.
h. ′
8 25 Leſs than ρ Perſei.
8 40 Ditto.
9 0 Equal to ρ, though Algol appeared rather brighter.
9 15 A little brighter than δ and ρ Perſei.

By taking a mean between 8 h. 25′ and 8 h. 40′, it appears, its leaſt brightneſs happened at 8 h. 32′; true to 10 minutes. The weather was rather hazy during ſome part of this obſervation.

November 17.

4 58 A little leſs than ρ Perſei.
5 15 Ditto.
5 35 Rather brighter than ρ Perſei.
5 50 A little brighter than ρ Perſei, but leſs than δ Perſei.
6 5 Rather brighter than δ Perſei.
6 40 Equal to β Trianguli, and brighter than ε and ζ Perſei.
7 20 A little brighter than β Arietis.
8 30 Between the ſecond and third magnitude, and equal to β Caſſiopeæ, but leſs than α and γ.
8 50 Second magnitude, and equal to γ Caſſiopeæ.
9 25 Nearly the ſame, if not rather brighter.

The variation has therefore ended at 9 h. 0′ nearly, and its leaſt brightneſs by taking a mean between 4 h. 58′ and 5 h. 15′, happened at 5 h. 7′; true, I believe, to 10 minutes. The weather was fine.

I have ſeveral more obſervations on Algol, where I have not been able to aſcertain its leaſt brightneſs, which all happened agreeable to the period as above determined; *viz.* May 20. July 5. and 22. Auguſt 14. September 6. 9. 12. and 26. October 2. 5. 19. and 22. and December 7.

XXIII. *Experiments and Obſervations on the* Terra Ponderoſa, &c. *By* William Withering, *M. D.; communicated by* Richard Kirwan, *Eſq. F. R. S.*

Read April 22, 1784.

SECTION I.

Terra ponderoſa aërata.

THIS ſubſtance was got out of a lead-mine at Alſton-Moor, in Cumberland. I firſt ſaw it in the valuable collection of my worthy and ingenious friend MATTHEW BOULTON, Eſq. at Soho; who, when he picked it up, conjectured from its weight that it contained ſomething metallic. About two years ago I ſaw it in his poſſeſſion; and partly from its appearance, being different from that of any calcareous ſpar I had ſeen, and partly from its great weight, I ſuſpected it to be the ſpatum ponderoſum.

A few experiments made at the moment confirmed my ſuſpicions, at leaſt ſo far as to ſhew that it contained a large proportion of the terra ponderoſa united to fixed air; but I did not then flatter myſelf that it would prove ſo pure as I afterwards found it to be.

Profeſſor BERGMAN, in his Sciagraphia Regni Mineralis, publiſhed laſt year at Leipſic, conjectures (§ 58.), with his uſual ſagacity, "Terra ponderoſa *nitrata* forte alicubi nativa "occurrit, a nemine tamen adhuc inventa, quod etiam valet "de *aërata*."

I was much delighted by the detection of a ſubſtance which promiſes to be of very conſiderable utility in chemical inquiries, and more ſo when I found it to be a native of this country; for it is not improbable, that it may be met with in many other mines, beſides that at Alſton-Moor.

Mr. BOULTON, with his uſual benevolence, preſented me with a piece of it, part of which accompanies this paper, for the inſpection of the Members of the Royal Society.

More obvious Properties.

Its general appearance is not much unlike that of a lump of alum; but, upon cloſer inſpection, it ſeems to be compoſed of ſlender ſpiculæ in cloſe contact, but more or leſs diverging. It may be cut with a knife. Its ſpecific gravity is from 4,300 to 4,338.

It effervesces with acids, and melts under the blow-pipe, though not very readily. Placed in a covered crucible, in a hot parlour fire, it loſt its tranſparency.

After expoſure to a moderate heat in a melting furnace, it adhered to the crucible, and exhibited ſigns of fuſion; but was not diminiſhed in weight, did not feel cauſtic when applied to the tongue, nor had it loſt its property of effervesſcing with acids.

Hence it is probable, that its loſs of tranſparency was rather occaſioned by numerous ſmall cracks, than by any eſcape of the water of cryſtallization, or of its aërial acid.

EXPERIMENTS.

A. 500 grains, diſſolved in muriatic acid, in ſuch a manner that nothing but elaſtic fluid could eſcape, loſt in ſolution 104 grains, and there remained an inſoluble reſiduum of nearly 3 grains.

2. In another experiment 100 grains loſt in ſolution 21 grains, and there remained 0,6 of a grain of inſoluble matter.

B. 100 grains diſſolving in dilute muriatic acid, gave but 25 ounce meaſures of air. This air was received in quickſilver, and when the ſpar was wholly diſſolved, the ſolution was boiled, in order to drive out what air might be lodged in it.

2. This air was heavier than atmoſpheric air; it was readily abſorbed by agitation in water, it precipitated lime from lime-water, and it extinguiſhed flame. The water which had abſorbed it changed the blue colour of litmus ſlowly* to a red; ſo that this elaſtic fluid was undoubtedly fixed air.

C. The ſolution (B) by the addition of mild foſſil fixed alkaly, afforded a precipitate which, after proper waſhing and drying, weighed 100 grains.

2. This precipitate, upon being again diſſolved in marine acid, yielded only 20 ounce meaſures of fixed air.

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* Other acids turn the blue of litmus inſtantly to a red, whilſt water impregnated with fixed air, does not change the litmus immediately; but, after ſome ſeconds, the red colour begins to appear, and then gradually grows more diſtinct.

D. To a ſaturated ſolution in marine acid mild fixed vegetable alkaly was added; the earth was precipitated, and a quantity of fixed air eſcaped.

2. The ſame thing happened when mild foſſil alkaly was added.

3. When cauſtic vegetable alkaly was uſed, the precipitation took place, but without any appearance of efferveſcence.

4. 50 parts diſſolved in marine acid loſt, during the ſolution, nearly 10,5. This ſolution, upon the addition of cauſtic vegetable alkaly, let fall a precipitate which, when waſhed and dried, weighed 45,5.

5. Phlogiſticated alkaly precipitated the whole of the earth from part of the ſolution D; for mild fixed alkaly afterwards added to the filtered liquor occaſioned no further precipitation.

E. Part of the precipitates D. 1. 2. after expoſure to a ſtrong heat in a crucible, was thrown into water. Next morning the water was completely covered with an ice-like cruſt, and had the acrid taſte of lime-water in a very high degree.

2. The ſmalleſt portion of vitriolic acid added to this water occaſioned an immediate and copious precipitation; and when this acrid water was diluted with 200 times its bulk of pure water, the precipitation upon the addition of vitriolic acid was yet ſufficiently obvious.

3. A ſingle drop of this acrid water, added to ſolutions of tartar of vitriol, GLAUBER's ſalt, vitriolic ammoniac, alum, Epſom ſalt, ſelenite, occaſioned an immediate precipitation in all of them.

F. The precipitate thrown down by the cauſtic vegetable alkaly (D. 3.) was put into water, in expectation that it would make lime-water; but neither upon ſtanding, nor after boiling, did this water exhibit any precipitation when concentrated vitriolic acid was dropped in it; nor had it any acrimonious or other peculiar taſte.

G. Concentrated vitriolic acid was added to one portion of the precipitate D. 3; concentrated nitrous acid to a ſecond portion; and marine acid to a third portion. No efferveſcence could be obſerved, nor was there any appearance of ſolution. After ſtanding one hour water was added; and the acids, thus diluted, were ſuffered to remain upon the portions of the precipitate for another hour. They were then decanted, and ſaturated with mild foſſil fixed alkaly, but without any appearance of precipitation.

H. The part precipitated by the phlogiſticated alkaly, when mixed with nitre and borax, and fluxed by a blow-pipe upon charcoal, formed a black glaſs; upon flint-glaſs, a white; and upon a tobacco-pipe an opaque yellowiſh white one.

2. Another portion melted with ſoap and borax in a crucible, formed a black glaſs, but without any metallic appearance.

I. The inſoluble reſiduum (A.) was boiled in water, the water decanted, and mild fixed alkaly added, but without any ſubſequent precipitation.

2. This inſoluble powder was not attacked by the nitrous or marine acids; but being put into vitriolic acid, and boiled a conſiderable time until the acid became highly concentrated, it diſſolved entirely, and ſeparated again upon the addition of water. It will appear in the ſequel, that the ſame thing happens to marmor metallicum, when diſſolved by boiling in the acid of vitriol.

CONCLUSIONS.

Hence it appears, that 100 parts of this ſpar contain

Terra ponderoſa pura	-	78,6
Marmor metallicum	-	,6
Fixed air	- - -	20,8
		100

OBSERVATIONS.

1ſt, The quantity of mild fixed alkaly neceſſary to ſaturate an acid, previouſly united to the terra ponderoſa, contains more fixed air than is neceſſary to ſaturate that quantity of terra ponderoſa D. 1. 2.

2dly, The terra ponderoſa, when precipitated from an acid by means of a mild fixed alkaly (D. 1. 2.), readily burns to lime; and this lime-water proves a very nice teſt of the preſence of vitriolic acid. E. 2. 3.

3dly, It is very remarkable, that the terra ponderoſa ſpar, in its native ſtate, will not burn to lime. In the lower degrees of heat it ſuffers no change, as was before obſerved, beſides the loſs of its tranſparency. When urged with a ſtronger fire, it melts and unites to the crucible, but does not become cauſtic. I buried it in charcoal-duſt in a covered crucible, and then expoſed it to a pretty ſtrong heat; but it did not part with its air.

May we not conjecture, then, that as cauſtic lime cannot unite to fixed air without the intervention of moiſture, and as this ſpar ſeems to contain no water in its compoſition, that it is the want of water which prevents the fixed air aſſuming its elaſtic aërial ſtate? This ſuppoſition becomes ſtill more probable, if we obſerve that when the ſolution of the ſpar in an acid is precipitated by a mild alkaly, C. 1. 2. ſome water enters into the compoſition of the precipitate, for it weighs the ſame as before it was diſſolved, and yet contains only 20 ounce meaſures of fixed air, whilſt the native ſpar contained 25 ounce meaſures; ſo that there is an addition of weight equal to that of 5 ounce meaſures of fixable air, or 3¼ grains to be accounted for, which can only ariſe from the water; and this precipitate, thus united to water, readily loſes its aërial acid in the fire, E. 1.

4thly, Profeſſor **Bergman** ſuppoſes the terra ponderoſa to be a metallic earth*; its entire ſeparation from an acid by means of the phlogiſticated alkaly (D. 5.) certainly favours ſuch a ſuppoſition; but, if it be ſo, it is evident from experiments H. 1. 2. that other means than thoſe commonly employed muſt be uſed to effect its reduction.

5thly, The precipitate made by the cauſtic vegetable alkaly D. 4. taking ſome of the alkaly down with it, and thus forming a ſubſtance neither ſoluble in water nor in acids, is a very curious phænomenon.

I afterwards varied the experiment by adding the terra ponderoſa lime-water (E.) to cauſtic vegetable and cauſtic foſſil alkaly. In both caſes this inſoluble compound was immediately formed; but not ſo when cauſtic volatile alkaly was uſed. This compoſition of an alkaly and an earth certainly deſerves more attention than I am at preſent able to beſtow upon it.

6thly, As it appears from experiments D. 1. 2. 3. 4. that fixed alkalies, both mild and cauſtic, ſeparated the terra ponderoſa from marine acid, I was at a loſs to know why Profeſſor BERGMAN, in his admirable table of ſimple elective attractions, placed the terra ponderoſa cauſtica immediately under the vitriolic, nitrous, and marine acids, and conſequently above the cauſtic alkalies. I was intereſted in the reality of the facts, becauſe I had ſo ſeldom ſeen reaſon to doubt the obſervations of that very excellent chemiſt, and therefore made the following experiments.

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* See preface to his Sciagraphia Regni Mineralis.

To different portions of terra ponderoſa ſalita and terra ponderoſa nitrata I added, drop by drop, cauſtic vegetable, cauſtic foſſil, and cauſtic volatile alkalies. In every caſe the EARTH was thrown down; and I have ſo often repeated theſe experiments to ſatisfy myſelf and others, that I am perſuaded the terra ponderoſa cauſtica ought to be placed below the alkalies, except in the column appropriated to the vitriolic acid; and it may be ſeparated even from that acid, by the vegetable fixed alkaly, if the alkaly be applied *via ſicca*, as will appear in the next ſection.

7thly, The neceſſity for uſing pure acids upon many occaſions, and the difficulty of obtaining them pure, are ſufficiently obvious. The VITRIOLIC ACID, as made in the large way, is generally pure enough for moſt purpoſes. It is apt to get coloured by inflammable matter; but this is ſeldom an inconvenience; and, when it would be ſo, it is eaſy to drive it off by boiling the acid in a Florence flaſk over a common fire. But there is another cauſe of impurity in this acid, which appears upon diluting it with water; for then it becomes milky, and in a ſhort time a powder ſubſides.*

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* About two years ago I examined this powdery matter; both that which was thrown

The acid may be freed from this powder either by diſtillation in glaſs veſſels, which is a tedious and dangerous proceſs, or by the affuſion of water; and, after the powder has ſubſided, a gentle evaporation will drive off moſt of the ſuperfluous water.

NITROUS ACID may be freed from vitriolic and marine acids, by ſolution of ſilver in the acid of nitre, as is daily practiſed; but the MARINE ACID has not, to my knowledge, been purified by any other method than the laborious one of re-diſtilling it from common ſalt. It is generally mixed with vitriolic acid, and often in large proportion. There is no temptation, and ſcarcely an opportunity, for it to be contaminated by nitrous acid. From the vitriolic acid then it may be readily purified by the addition of terra ponderoſa cauſtica diſſolved in water, or by the terra ponderoſa ſalita. If the latter be uſed, a ſmall

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thrown down by dilution with water, and alſo ſome which Dr. PRIESTLEY gave me, being the reſiduum of vitriolic acid diſtilled to dryneſs in a flint-glaſs retort.

1ſt, Repeated boiling in water, reduced 6½ grains to 2 grains.

2dly, This ſolution, by gentle evaporation, afforded 5 grains of cryſtals, as hard and as taſteleſs as ſelenite.

3dly, To theſe cryſtals, re-diſſolved in water, mild foſſil alkaly was added, and a white powder precipitated.

4thly, This powder, after expoſure to a pretty ſharp heat, was thrown into water; part of it diſſolved, and the water got the taſte and other properties of lime-water.

5thly, The inſoluble part (1.) ſuffered no change by boiling in nitrous acid; one-half of it mixed with borax, and expoſed to the blow-pipe upon charcoal, vitrified; the other half, mixed with borax and charcoal-duſt, likewiſe vitrified.

CONCLUSIONS. It appears, then, that the greater part of this ſubſtance was calx vitriolata, or ſelenite; the remainder a vitrifiable earth.

I had before found, that the terra ponderoſa vitriolata, or heavy gypſum, would diſſolve in concentrated vitriolic acid; but always ſeparated in a powdery form upon the affuſion of water; and now it appears, that calx vitriolata, or ſelenite, does the ſame.

portion of the acid muſt firſt be tried in a diluted ſtate, from whence we muſt judge how much of the terra ponderoſa ſalita the whole will require; or elſe the whole of the acid muſt be diluted with water. Whether we uſe the terra ponderoſa diſſolved in water or in marine acid; in either caſe the acid of vitriol immediately ſeizes upon it, and ſubſides with it in form of an inſoluble powder.

As there are reaſons for preferring the marine acid in ſeveral of the nicer and more important enquiries of chemiſtry, this ready method of purifying it cannot but prove acceptable.

SECTION II.

Terra ponderoſa vitriolata. BERGMAN's Sciagraphia, §§ 58. 89.

Variety, Heavy Gypſum. Ponderous Spar.

Marmor Metallicum. CRONSTEDT Min. § 18. 2. 19. C.

From Kilpatrick-hills near Glaſgow. A ſort, with ſmaller cryſtals, amongſt the iron ore about Ketley in Shropſhire. In the lead mines at Alſton-Moor.

More obvious Properties.

White; nearly tranſparent, but has not the property of double refraction; compoſed of laminæ of rhomboidal cryſtals; decrepitates in the fire. Specific gravity from 4,402 to 4,440.

EXPERIMENTS.

A. 100 grains expoſed to a red heat for one hour, in a black lead crucible, loſt five grains in weight; but as a ſulphureous ſmell was perceptible, I ſuſpected that a decompoſition had taken place, and therefore expoſed another portion to a ſimilar heat for the ſame ſpace of time in a tobacco-pipe. This had no ſmell of ſulphur, nor was it diminiſhed in weight.

2. It is barely fuſible under the blow-pipe; but with borax fluxes readily into a white opake glaſs.

B. 100 grains, ground in a mortar, and waſhed over extremely fine by repeated additions of water, were boiled in the ſame water, and, after ſettling, the water was poured off. The powder, when dried, had not ſenſibly loſt weight.

2. To ſeparate portions of the waſhing water, were added mild vegetable and mild foſſil alkaly; but without any appearance of precipitation. Nitre of mercury gave a very ſlight browniſh cloud, barely diſcernible; and nitre of ſilver an extremely ſlight bluiſh appearance.

3. The ſame powder, boiled again in freſh water, did not affect the water at all; for it ſtood the teſt of nitre of ſilver without any change.

C. Portions of the powder B. were boiled in vitriolic, nitrous, and muriatic acids, of the uſual ſtrength, for ſeveral minutes. The acids were then ſaturated with vegetable fixed alkaly, but without any appearance of precipitation, nor had the portions of powder loſt any weight.

2. But when boiled in vitriolic acid, until that acid became very much concentrated and nearly red-hot, the whole of it diſſolved; but, ſeparated again upon the addition of water, was

not altered in its weight, was not acted upon by acids of the uſual ſtrength, and had, under the blow-pipe, the properties mentioned at A. 2.

3. Some of the ſolution in the concentrated vitriolic acid was left expoſed to the atmoſphere, that the acid might ſlowly attract water. After ſome days, beautiful cryſtals appeared in the ſhapes of ſtars, faſciæ, and other radiated forms.

4. To another portion of this ſolution mild fixed vegetable alkaly was added; but the precipitate appeared to be the marmor metallicum unchanged.

D. One ounce of this marmor metallicum in fine powder was fluxed in a crucible with two ounces of ſalt of tartar, until it ran thin. This ſubſtance, boiled with water in a Florence flaſk, left a reſiduum of ſix drams.

E. This reſiduum was thrown into water, and pure nitrous acid added, until there was no more efferveſcence. The undiſſolved part weighed 52 grains.

F. This undiſſolved part appeared to be the original ſubſtance no ways changed; for it did not diſſolve in nitrous or marine acids, but did wholly diſſolve in the greatly concentrated and boiling vitriolic acid, from which it was again ſeparated by the addition of water. (C. 2.)

G. The ſolution D. was ſaturated with diſtilled vinegar, and then evaporated to dryneſs, but with leſs than a boiling heat. The ſal diureticus, thus formed, was waſhed away with alcohol. The remaining ſalt weighed 5 drams nearly.

2. This ſalt had the appearance and the taſte of vitriolated tartar; it decrepitated in the fire; roaſted with charcoal-duſt, it formed a hepar ſulphuris; and with muria calcarea gave a precipitation of ſelenite.

H. The ſalt, formed with the nitrous acid (E), ſhot readily into beautiful permanent cryſtals, of a rough bitteriſh taſte.

2. Some of this ſalt was deflagrated with nitre and charcoal, and the alkaly afterwards waſhed away.

3. The reſiduum, being the earth of the marmor metallicum, was very white, burnt to lime, and again formed an inſoluble compound with acid of vitriol.

I. 100 grains of terra ponderoſa aërata were diſſolved in diluted marine acid. Vitriolic acid was dropped into this ſolution, until no more precipitation enſued. The precipitate, after very careful waſhing and drying, was expoſed to a red heat in a covered tobacco-pipe for half an hour: when cool, it weighed 117 grains.

2. 50 grains of terra ponderoſa aërata in a lump were put into diluted vitriolic acid; but the action of the acid upon it was hardly ſenſible, even when made hot.

Marine acid was then added, and after ſome time an effervеſcence appeared. The terra ponderoſa vitriolata, thus formed, after proper waſhing and drying, was expoſed to a red heat for an hour: it then weighed 58,4 grains.

CONCLUSIONS.

1ſt, It appears that the marmor metallicum is compoſed of vitriolic acid and terra ponderoſa, D. E. F. G. H.

2dly, That this compound, though probably ſoluble in water, has ſo little ſolubility as almoſt to eſcape detection by the niceſt chemical teſts, B. 1. 2. 3.

3dly, That it is not ſoluble in acids of the uſual ſtrength; but that it perfectly and entirely diſſolves in highly concentrated vitriolic acid, from which it again ſeparates entire and unchanged upon the affuſion of water, C. 1. 2.

4thly, That it cannot be decompoſed (*via humida*) by mild fixed alkaly, C. 4.

5thly, That it may be decompoſed (*via ſicca*) by the vegetable fixed alkaly, D. E. G. H.

6thly, That it may be decompoſed by inflammable matter, uniting to its acid, and forming ſulphur; but that it cannot be decompoſed by heat alone, A. 1.

7thly, From experiments I. 1. 2. it appears, that 100 parts of this ſubſtance contain

Vitriolic acid pure -	32,8
Terra ponderoſa pure	67,2
	100

For the 100 parts of terra ponderoſa aërata made uſe of in the experiment I. 1. would loſe during the ſolution 20,8 of fixed air (§ 1ſt, A.); then, deducting 0,6 for the marmor metallicum contained in the terra ponderoſa aërata (§ 1ſt. A. 1. 2.), there remains 78,6 of pure terra ponderoſa. This, when ſaturated with vitriolic acid, and made perfectly dry, weighed 117; conſequently it had taken 38,4 of vitriolic acid.

OBSERVATIONS.

The apparent inſolubility of terra ponderoſa aërata in the diluted vitriolic acid (I. 2.) can be accounted for by remarking, that the moment the ſurface of the lump was acted upon by the acid, an inſoluble coat of marmor metallicum was formed upon it, which effectually precluded any further operation of the acid.

Profeſſor **Bergman**, in order to obtain the earth from the terra ponderoſa vitriolata, directs the latter to be roaſted with fixed alkaly, and the duſt of charcoal; but I have always done it by charcoal duſt alone, though probably this method may require a greater degree of heat.

It has been remarked, that terra ponderoſa and calx of lead reſemble each other in many reſpects; and I muſt add, that the vitriol of lead diſſolves in the highly concentrated vitriolic acid much in the ſame manner that the marmor metallicum does, and like this too ſeparates upon the affuſion of water; but I never obſerved it diſpoſed to cryſtallize.

The marmor metallicum may probably be uſeful in ſome caſes where a powerful flux is wanted; for I once mixed ſome of it with the black flux, and expoſing it to a pretty ſharp heat, it entirely ran through the crucible. May not, therefore, ſome of the more common varieties of it be uſed advantageouſly as a flux to ſome of the more refractory metallic ores?

SECTION III.

Terra ponderoſa Vitriolata.
Variety, Calk or Cauk.
Marmor Metallicum, **Cronstedt** Min. § 18. B?

Plentiful in the Mines in Derbyſhire.

More obvious Properties.

Of a white or reddiſh colour; cryſtallized in rhomboidal laminæ, but theſe very much intermixed and confuſed. Loſes little or nothing of its weight by being made red-hot. Specific gravity 4,330.

EXPERIMENTS.

A. Ground in a mortar, and waſhed over, the waſhing water, when decanted, gave no precipitation with mild vegetable alkaly; but with nitre of ſilver and nitre of mercury the ſlighteſt cloud imaginable.

B. 100 grains boiled in marine acid weighed, after proper waſhing and drying, 99,5.

C. The acid ſolution B let fall a Pruſſian blue upon the addition of a ſingle drop of phlogiſticated fixed alkaly; and, when ſaturated with mild foſſil alkaly, afforded an ochry-coloured precipitate.

D. This precipitate, collected and waſhed, weighed half a grain. It was roaſted with tallow, and then was wholly attracted by a magnet.

E. A quantity of the cauk, finely powdered, was mixed with charcoal-duſt, and roaſted in a crucible at a white heat, for five hours, freſh charcoal-duſt being occaſionally added. It gave out a ſtrong ſmell of ſulphur.

F. To this roaſted cauk nitrous acid was added, which diſſolved the greater part of it; producing, during the ſolution, ſome effervеſcence, and a ſtrong ſmell of hepar ſulphuris.

G. Some of this ſolution, after proper evaporation, afforded beautiful cryſtals, not deliqueſcent, exactly reſembling thoſe obtained from the marmor metallicum, (§ II. H.).

H. To other portions of the ſolution F, were added fixed vegetable and foſſil alkalies, and likewiſe volatile alkaly, each of which precipitated the earth from the acid.

I. This earth, after expoſure to a white heat for one hour, became cauſtic, and made lime-water, ſimilar in properties to to that mentioned at § Iſt. E.

K. Some of the part not acted upon by the nitrous acid F, diſſolved entirely by boiling in highly concentrated vitriolic acid, and wholly ſeparated again by the affuſion of water. More water was added, and the whole was boiled again; but the filtered liquor gave no ſigns of precipitation upon the moſt liberal addition of mild fixed vegetable alkaly.

CONCLUSIONS.

It appears, therefore, that 100 parts of Derbyſhire cauk contain

Marmor metallicum -	99,5
Calciform iron - -	,5
	100

And it is probable, that the redder pieces contain a little more iron.

SECTION IV.

Terra ponderoſa vitriolata.

Variety, radiated Cauk.

Gypſum cryſtalliſatum capillare. CRONSTEDT Min. § 19. B.

From Pennely by the Bog, near Minſterley, in Shropſhire, fifteen miles from Salop, on the road to Montgomery.

More

More obvious Properties.

Somewhat gloſſy like ſatin; yellowiſh-white, opake; compoſed of ſlender ſpiculæ ſet cloſe together, and pointing from a center.

In ſome pieces there are concentric circles of a ſemi-tranſparent horn like appearance. It is not very brittle; may be ſhaved with a knife; loſes little or nothing of its weight by being made red-hot. Its ſpecific gravity 4,000; but after ſoaking one night in the water 4,200, or more.

EXPERIMENTS.

When treated in the ſame manner that the Derbyſhire cauk was, in the preceding ſection, 100 parts of it appeared to contain

Marmor metallicum	-	97,7
Calciform iron	- -	2,3
		100

Suſpecting that the preſence of ſo ſmall a proportion of iron could hardly occaſion the whole of the apparent differences betwixt the Shropſhire and Derbyſhire cauks and the marmor metallicum; and thinking it not improbable, that they might contain lead; I mixed ſome of them with charcoal-duſt and borax, but could not by means of the blow-pipe produce any metallic appearance, although vitriol of lead, treated in the ſame manner, was readily reduced.

I then mixed four parts of cauk with one part of vitriol of lead; the lead could ſtill be reduced, though not ſo readily as before.

GENERAL OBSERVATIONS.

The terra ponderoſa ſeems to claim a place betwixt the earths and the metallic calces. Like the former, it cannot be made to aſſume a metallic form; but, like the latter, it may be precipitated from an acid, by means of phlogiſticated alkaly. In many of its properties it much reſembles the calx of lead; and in others, the common calcareous earth, but ſtill ſeems ſufficiently different from that to conſtitute a new genus, as will appear from a little attention to the following circumſtances.

Terra ponderoſa,	Terra calcarea,
When diſſolved in water, precipitates upon the addition of the ſmalleſt portion of vitriolic acid.	Diſſolved in water, does not precipitate upon the addition of vitriolic acid.
Its gypſum, therefore, is inſoluble.	Its gypſum, therefore, is ſoluble.
With the nitrous and marine acid, forms cryſtals which do not deliqueſce.	With nitrous and marine acids forms ſalts ſo deliqueſcent that they cannot be kept in a cryſtallized form.
Decompoſes vitriolic ſalts *via humida*.	Does not decompoſe vitriolic ſalts.

It has been called terra ponderoſa, or heavy earth, upon account of the great ſpecific gravity of its gypſum; its ſpar is likewiſe heavy enough to countenance ſuch an appellation; but the earth itſelf does not appear to be a heavy ſubſtance, and I imagine the great weight of its compounds with the vitriolic and aërial acids is owing to the abſence of water.

Birmingham, Nov. 1783.

XXIV. *Obſervations du Paſſage de* Mercure *ſur le Diſque du Soleil le* 12 *Novembre,* 1782, *faites à l' Obſervatoire Royal de* Paris, *avec des réflexions ſur un effet qui ſe fait ſentir dans ces mêmes Obſervations ſemblable à celui d'une Réfraction dans l'Atmoſphere de* Mercure. *Par* Johann Wilhelm Wallot, *Membre de l'Académie Électorale de Sciences et Belles Lettres de* Manheim, *&c. Communicated by* Joſeph Planta, *Eſq. Sec. R. S.*

Read April 29, 1784.

1. LES paſſages de Mercure ſur le diſque du ſoleil ſont d'autant plus intéreſſans pour les aſtronomes, qu'ils donnent principalement le moyen de déterminer avec plus d'exactitude la poſition des nœuds de ſon orbite, et que la difficulté de voir cette planète dans ſes autres aſpects avec le ſoleil en rend les obſervations plus précieuſes.

2. Deux circonſtances aſſez déſavantageuſes qui devaient accompagner particulierement le paſſage dont il s'agit ici, ſavoir la proximité du ſoleil de l'horizon, et Mercure paſſant trop près du bord de cet aſtre, ſemblaient par leur nature offrir trop d'inconvéniens pour en eſpérer des obſervations bien exactes; cependant l'encouragement qu'a donné le beau tems qu'il fit toute la journée du 12 Novembre, nous ayant fait apporter une plus grande attention aux obſervations, nous autoriſe maintenant à en avoir une meilleure opinion. Je crois pouvoir aſſurer ſans oſtentation d'y avoir réuſſi aſſez pour être ſatisfait des miennes, et pour oſer les garantir autant que la nature des

choſes peut le permettre. Si je puis me flatter d'avoir obtenu de ce paſſage une obſervation très exacte et peut-être la plus complette, je ne diſſimulerai pas que je dois en grande partie cet avantage à M. DE CASSINI qui, m'ayant laiſſé la meilleure lunette * qu'il y ait à l'Obſervatoire Royal, m'avait mis par là dans le cas d'employer la plus grande vigilance pour mériter par l'exactitude de mes opérations la confiance qu'on me témoignait dans une occaſion auſſi importante.

3. Nous avons fait (M. DE CASSINI et moi) toutes les obſervations néceſſaires pour conſtater avec la plus grande exactitude l'état de notre pendule; et, en réduiſant mes obſervations au tems vrai, je n'ai pas même négligé les dixièmes de ſeconde. Cette préciſion ſcrupuleuſe paraitra peut-être ſuperflue dans de pareilles obſervations, mais on verra par la ſuite de ce Mémoire les raiſons qui m'y ont déterminé. Voici mes obſervations dans le même ordre où elles ſe ſont faites, et réduites au tems vrai de la méridienne de l'Obſervatoire Royal de Paris.

	Tems vrai. h.	′	″	
Entrée	à 2	56	28,8	Je ſoupçonne la planète. Contact extérieur de l'entrée.
	à 2	58	28,8	J'eſtime Mercure entré à moitié. Centre de ☿ ſur le bord du ☉.
	à 3	2	3,8	Contact intérieur de l'entrée.
	à 3	3	45,8	Mercure abſolument détaché du ſoleil.

En méſurant le diamètre de Mercure ſur le diſque du ſoleil je l'ai trouvé par deux fois exactement de la même quantité, ſavoir de 9 parties du micromètre objectif, qui valent 9″,535 de degrés du grand cercle.

		′	″	
Sortie	à 4	17	18,4	Contact intérieur de la ſortie.
	à 4	20	36,4	Le centre de Mercure ſur le bord du ſoleil.
	à 4	22	53,4	Contact extérieur de la ſortie. Mercure totalement perdu de vue.

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* Une excellente lunette achromatique de DOLLOND de 3 pieds.

Le bord du ſoleil était ſi ondoyant que Mercure, aux approches de ſa ſortie totale, reſſemblait exactement à un corps flottant ſur les vagues d'une eau fortement agitée, et qui tantôt diſparait entièrement, tantôt élevé par les vagues ſe montre en partie et quelquefois tout entier. Ces vagues ou ondulations allaient toujours dans le même ſens du N. Oueſt au Sud Eſt. Leur mouvement était aſſez rapide, et c'eſt préciſément la rapidité de ce mouvement qui m'a ſingulièrement favoriſé l'obſervation du contact extérieur de la ſortie de Mercure, parceque je ne la perdais jamais de vue qu'un inſtant.

Je terminerai le détail de mes obſervations par aſſurer, que je n'ai pas apperçu la moindre apparence d'une atmoſphère ou nébuloſité autour de Mercure pendant toute la durée de ſon paſſage, quoique la lunette me repréſentât tous les objets très diſtinctement. J'ai toujours vu le diſque de Mercure bien noir, et également bien terminé dans toute ſa circonférence qui me paraiſſait toujours tranchée nette, ſurtout dans le commencement où les ondulations étaient moins fortes juſques vers le milieu du paſſage. Mais cela ne m'empêchera pas d'être très perſuadé de l'exiſtence d'une atmoſphère autour de Mercure, comme autour de tous les corps céleſtes, et qu'on peut fort bien l'avoir apperçue dans ce paſſage ſous un ciel plus pur et plus beau que celui de Paris.

Réſultats du calcul des obſervations précédentes ſelon leurs différentes combinaiſons.

4. La methode que j'ai ſuivie pour réduire les obſervations de ce paſſage au centre de la terre, m'eſt en quelque ſorte particulière; mais comme elle n'eſt pas entièrement nouvelle puiſqu'elle

qu'elle ne diffère de toutes les methodes connues qu'en ce que je l'ai ſimplifiée en la rendant abſolument directe, je me contenterai d'en donner une idée générale. Je n'ai employé dans mes calculs que ce qui eſt donné directement par obſervation, ou bien des quantités plus exactement données par les tables, telles que le diamètre du ſoleil, ſon mouvement horaire et celui de Mercure. Mais ce qui caractériſe eſſentiellement cette méthode, c'eſt qu'en combinant les obſervations toujours enſemble deux à deux, on a la durée ou le tems écoulé d'une obſervation à l'autre qui eſt une des principales données du problême, et la plus exacte qu'on puiſſe ſe procurer par obſervation. Or, quand l'obſervation nous fournit directement des données exactes, je ne vois abſolument pas la néceſſité d'en aller chercher de moins exactes pour les faire entrer dans le calcul. C'eſt pourtant ce que font quelques aſtronomes modernes *, qui, en recommendant dans leur Traité d'Aſtronomie de calculer les obſervations ſéparément afin, diſent-ils, de multiplier les réſultats et d'en déduire plus exactement par un milieu la quantité qu'on cherche, ſont obligés pour cet effet de ſuppoſer à peu près connu le milieu du paſſage et la plus courte diſtance des centres †. Ce raiſonnement, auſſi éloigné des principes de la géometrie, que des regles de l'analyſe, me parait encore illuſoire quant à l'exactitude qu'on eſpère obtenir de la multiplicité des réſultats ainſi déterminés ; voici pourquoi.

5. Je ſuppoſe pour un inſtant qu'on prenne au hazard deux obſervations, et qu'on les calcule ſeparément chacun ſuivant cet précepte ; il eſt certain que ſi l'on ne ſuppoſe pas le milieu du paſſage et la plus courte diſtance des centres tels que les don-

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* Principalement M. De la Lande dans ſon Traité d'Aſtronomie, édition de 1771, livre XI. art. 2152.

† Ibid. art. 2062 et 2063.

ne prient directement ces deux obſervations combinées enſemble, on doit trouver, pour la quantité qu'on cherche, deux réſultats différens, et qui différeront d'autant plus que la ſuppoſition qu'on aura faite ſera plus éloignée de la véritable. On prend donc alors un milieu entre les deux réſultats et l'on s'imagine avoir trouvé la vérite; mais il me ſemble qu'il eſt très permis d'en douter, car, outre qu'il y á bien des cas où l'on ne peut pas regarder le réſultat moyen comme le véritable, ici ce n'eſt pas même admiſſible, puiſque le milieu du paſſage et la plus courte diſtance des centres ſont deux quantités qui dependent l'une de l'autre, et qu'il eſt impoſſible de les ſuppoſer telles préciſément qu'elles ſe conviennent relativement à deux obſervations déterminées, à moins que ce ne ſoit un effet du hazard. Or ſi je ſuppoſe maintenant qu'on prenne les deux mêmes obſervations, et qu'on les combine enſemble, il eſt clair qu'on ne trouvera qu'un ſeul réſultat pour la quantité cherchée, mais ce ſera préciſément la même qu'on aurait eue par un milieu entre les deux réſultats trouvés ſuivant l'autre manière ſi l'on y avait fait une ſuppoſition qui s'écartât peu de celle qu'il convenait de faire. Il s'en ſuit donc qu'on ſerait arrivé au même but par les deux méthodes, mais avec cette différence que les quantités déterminées d'après la méthode des combinaiſons ſont dans tous les cas de vrais réſultats tels que les donne véritablement l'obſervation, tandiſque d'après l'autre ce ne ſont que des réſultats fictifs ou approchés. Le calcul devient à la vérité plus long, lorſqu'il y a plus de trois obſervations; parceque alors le nombre des combinaiſons qu'on en peut faire deux à deux, conſéquemment auſſi le nombre des réſultats qui en proviennent, ſurpaſſera toujours celui des obſervations. Or ſi pour déterminer une quantité quelconque d'après une méthode on riſque de trouver des réſultats inexacts, et que d'après une autre méthode on peut déterminer la même quantité ſans courir ce danger, il eſt inconteſtable que celle-ci eſt préférable à l'autre. Lorſqu'on ne peut avoir que des obſervations iſolées, il faut bien alors ſe réſoudre à les calculer ſéparément, mais encore avec la reſtriction que les quantités qu'on ſuppoſera connues ſoient données par d'autres obſervations, qui étant dans le cas d'être combinées deux à deux, ſoient elles-mêmes très exactes. Il eſt donc aiſé de conclure de tout ce que je viens de dire que la manière de calculer ſéparément chaque obſervation, non ſeulement ne procure pas les avantages qu'on en attend pour la multiplicité des réſultats, mais elle eſt encore moins exacte que celle de combiner deux à deux les obſervations, ainſi que l'enſeignent les plus célèbres aſtronomes. Je ne me ſuis permis d'entrer dans ces détails que pour prouver à la Société Royale que je ne me ſers jamais avec confiance d'aucune méthode ſans l'avoir examinée auparavant en la créant, pour ainſi dire, une ſeconde fois.

6. J'ai calculé le lieu du ſoleil et de Mercure par les tables de HALLEY pour 2½ h. 3½ h. et 4½ h. eſpace de tems qui comprend à peu près par ſon milieu toute la durée du paſſage, et j'ai trouvé,

	à 2h30' tems vrai				à 3 h. 30' t. v.				à 4 h. 30' t. v.			
	S.	°	'	''	S.	h.	'	''	S.	°	'	''
La longitude du ſoleil de	7	20	22	43,6	7	20	25	14,8	7	20	27	45,9
Son aſcenſion droite	7	17	55	55,3	7	17	58	28,4	7	18	1	1,5
Sa déclinaiſon - - auſtrale		17	51	49,6		17	52	29,9		17	53	10,1
La longit. géocentrique de Mercure	7	20	32	2,9	7	20	28	40,8	7	20	25	18,4
Sa latitude - - - boréale		0	14	31,0		0	15	22,6		0	16	13,8

Ce qui me donne	entre 2½ h. et 3½ h			entre 3½ h. et 4½ h		
	°	'	''	°	'	''
Le mouvem horaire relatif Merc. ſur l'écliptique de		5	33, 3		5	53, 5
L'inclinaiſon de l'orbite relative ſur l'écliptique de	8	18	33, 8	8	14	28, 5
Le mouvem. horaire relatif de Merc. dans ſon orbite		5	57,05		5	57,19

Je me ſuis ſervi de l'inclinaiſon et du mouvement horaire qui avait lieu entre 2½ h. et 3½ h. dans le calcul des obſervations du commencement, et l'inclinaiſon avec le mouvement horaire qui avait lieu entre 3½ h. et 4½ h. m'a ſervi pour la fin du paſſage. Quant aux autres élémens, j'ai employé le diamètre du ſoleil de 32′ 24″,5 ; celui de Mercure de 9″,535 comme je l'ai méſuré ſur le diſque du ſoleil pendant le paſſage, et la parallaxe horizontale du ſoleil dans ſes moyennes diſtances de 8″,9 telle que je l'ai établie dans mon Mémoire ſur le paſſage de Venus en 1769. D'où j'ai conclu la différence des parallaxes horizontales du ſoleil et de Mercure pour le jour du paſſage 12 Novembre de 4″,088.

7. Avec ces éléments j'ai calculé les obſervations des contacts en ne négligeant pas même les millièmes de ſeconde dans certains cas ; je n'ai mis cette ſcrupuleuſe exactitude dans tous mes calculs que parceque je voulais m'aſſurer dans le cas où je viendrais à trouver des différences entre les réſultats de même dénomination que je n'euſſe à les attribuer uniquement qu'aux obſervations. La table ſuivante renfèrme les réſultats les plus importans de ces calculs.

Table des réſultats du calcul des obſervations des contacts et du centre de Mercure.

		Contacts intérieurs	Contacts extér.	Centre de ☿.
		h. ′ ″	h. ′ ″	h. ′ ″
Heure vraie de l'obſervation	entrée	3 2 3, 8	2 56 28, 8	2 58 28,8
	ſortie	4 17 18, 4	4 22 53, 4	4 20 36,4
Durée donnée directement par l'obſ.		1 15 14, 6	1 26 24, 6	1 22 7,6
Plus courte diſtance des centres vue à la ſurface de la terre		15 41, 2	15 42, 5	15 41,0
Heure vraie du milieu du paſſage pour le centre de la terre		3 39 47, 4	3 39 47, 1	3 39 38,7
Plus courte diſtance des centres vue du centre de la terre		15 45, 1	15 46, 4	15 44,9
Reduction de l'obſervation au centre de la terre	entrée	+ 2 59,45	+ 2 34,38	+ 2 42,9
	ſortie	− 2 46,70	− 2 22,27	− 2 30,7
Heure vr. de l'obſervation arrivée pour le centre ♁	entrée	3 5 3,25	2 59 3,11	3 1 11,7
	ſortie	4 14 31,70	4 20 31,13	4 18 5,7
Durée du paſſage pour le centre de la terre		1 9 28,45	1 21 28,02	1 16 54,0
Heure vraie de la conjonction de Mercure et du ſoleil		4 2 53, 2	4 2 54, 8	4 2 44,1
		S. ° ′ ″	S. ° ′ ″	S. ° ′ ″
Latitude de ☿ en conjonction donnée par obſervation		15 55,1	15 56,4	15 54,8
Longitude du ſoleil ou de Mercure en conjonction		7 20 26 37,6	7 20 26 37,7	7 20 26 37,2
Longitude de Mercure en conjonction donnée par les tables eu égard à l'aberration		7 20 27 8,4	7 20 27 8,3	7 20 27 8,9.
Latitude de ☿ en ☌ donnée par les tables - - - boréale		15 50,7	15 50,7	15 50,5
Erreur des tables	en longitude	−30,8	−30,6	−31,7
	en latitude	+ 4,4	+ 5,7	+ 4,4

En adoptant la latitude de ☿ au moment de la ☌ donnée par les contacts intérieurs de 15′ 55″,1 je trouve le lieu du ☊ de ☿ à

S. ° ′ ″
1 15 45 22,8 en ſuppoſant l'inclinaiſon de l'orbite 7° 0′ 0″ avec M. Cassini.
1 15 44 55,7 en ſuppoſant l'inclinaiſon de l'orbite 6° 59′ 20″ avec Halley.

8. L'on voit par cette table que les contacts intérieurs donnent l'heure du milieu du paſſage à 3 dixièmes de ſeconde près la même que les contacts extérieurs; l'heure de la conjonction à 1″,6 près la même*, et la plus courte diſtance des centres ainſi que la latitude de Mercure en conjonction de 1″,3 plus petite. Quant aux deux obſervations du centre de Mercure ſur les bords du ſoleil, elles donnent le milieu du paſſage de 8″,7 plutôt que les contacts intérieurs, et la plus courte diſtance des centres ainſi que la latitude de Mercure en conjonction de 2 dixièmes de ſeconde ſeulement plus petite. Cette différence dans l'heure du milieu du paſſage ne peut venir que de la manière dont j'ai eſtimé le centre de Mercure; car il y a d'abord ſeconde pour ſeconde le même intervalle de tems entre les deux contacts de l'entrée qu'entre ceux de la ſortie, c'eſt à dire l'un et l'autre de 5′ 35″. Enſuite je trouve qu'il s'était écoulé 2′ 0″ depuis le contact extérieur de l'entrée juſqu'au moment où j'ai eſtimé le centre de Mercure ſur le bord du ſoleil, au lieu de 2′ 17″ qu'il y a entre les pareilles obſervations de la ſortie; mais cet intervalle de tems devant être le même pour l'entrée et pour la ſortie, la différence 17″ fait voir que j'ai eſtimé le centre de Mercure plus près du contact extérieur à l'entrée qu'à la ſortie, ce qui devait auſſi avancer l'inſtant du milieu du paſſage; or la moitié de ces 17″ fait préciſément les 8′ ½ dont le milieu du paſſage eſt arrivé plutôt ſelon cette obſervation que ſelon celle des contacts intérieurs (puiſque l'erreur de l'une des deux obſervations n'eſt que la moitié ſur le milieu du paſſage). J'ai donc marqué l'inſtant de l'obſervation du centre à l'entrée plutôt qu'il

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* L'inſtant de la conjonction diffère de 1″,6 quoique celui du milieu du paſſage ne diffère que de 0″,3, parceque la portion de l'orbite relative compriſe entre le milieu du paſſage et la conjonction eſt plus grande pour une plus grande diſtance des centres.

ne fallait; car je pencherai toujours à croire plutôt que c'eſt ſur celle de l'entrée que doit tomber l'erreur, parceque n'ayant pas encore vu Mercure ſur le diſque du ſoleil, je ne pouvais pas juger de ſa grandeur auſſi bien qu'à la fin après l'avoir vu pendant toute la durée de ſon paſſage. C'eſt auſſi en partie par cette même raiſon, jointe à celle qu'on ne peut pas eſtimer avec quelque préciſion le centre d'un corps qu'on ne voit pas entièrement, que je puis avoir obſervé le centre de Mercure ſur le bord du ſoleil trop tôt à l'entrée, et trop tard à la ſortie relativement aux obſervations des contacts. Cette diſcuſſion, en apparence d'ailleurs peu importante, devient ici d'une grande néceſſité, parcequ'il s'agit de montrer les défauts de deux obſervations que je ne rejette qu'avec beaucoup de regrets; car l'obſervation du centre de la planète ſur le bord du ſoleil n'étant pas affectée de l'effet de pluſieurs élémens (le diamètre de la planète et l'effet d'une atmoſphère qui l'envelopperait) que nous connaiſſons ſouvent mal, ou que nous ignorons abſolument, offrirait des avantages réels, ſi elle pouvait ſe faire avec une certaine préciſion.

9. Quoique les réſultats de mes calculs s'accordent aſſez pour inſpirer quelque confiance, je n'ai cependant pas été trop ſatiſfait de trouver la plus courte diſtance des centres de $1'',3$ plus grande par les contacts extérieurs que par les contacts intérieurs. Cette différence annonce une erreur dans les durées. Ou la durée du paſſage entre les deux contacts extérieurs eſt trop petite, ou celle des contacts intérieurs eſt trop grande. Mais je me ſuis impoſé la loi de ne jamais faire aucune correction à mes obſervations lorſque je ne les ai accompagnées d'aucune marque qui me faſſe douter de leur bonté; je ne trouve donc aucune raiſon qui m'autoriſe à changer la durée des contacts intérieurs, et quand je voudrais m'écarter ici un moment de mes

principes pour augmenter la durée des contacts extérieurs, je ne le pourrais faire qu'en conſidération de l'incertitude avec laquelle on peut eſtimer le contact extérieur de l'entrée trop tard, et celui de la ſortie trop tôt, ce qui eſt toujours probable ; mais je ne la pourrais augmenter que tout au plus de 5 à 6 ſecondes de tems, puiſqu'on a vu dans l'article précédent qu'il n'y a que 17″ d'incertitude ſur l'eſtime des deux obſervations du centre de Mercure ſur les bords du ſoleil qui comparativement entre elles-mêmes ſe font beaucoup moins exactement. Or ces 5 ou 6 ſecondes d'augmentation ſur la durée extérieure ne ſuffiſent pas à beaucoup près (car il en faudrait 106″) pour réduire à zéro la différence qui ſe trouve entre les deux valeurs de la plus courte diſtance des centres. Il faut donc chercher ailleurs que dans les obſervations la cauſe de cette différence. C'eſt ce que je crois pouvoir trouver dans l'effét d'une atmoſphère ſuppoſée autour de Mercure, ou d'une cauſe ſemblable.

10. D'après les recherches que j'ai faites ſur l'atmoſphère de Venus à l'occaſion de ſon paſſage en 1769, et dont j'ai établi et démontré les principes dans un petit Traité complet ſur les paſſages de Venus et de Mercure, j'étais prévenu que la circonſtance caractériſtique de ce paſſage de Mercure qui était ſi deſavantageuſe à l'égard de l'utilité qu'on en retire pour perfectionner les tables, devait être extrêmement favorable à la détermination de l'effet d'une atmoſphère qui environnerait Mercure, puiſque la planète paſſant fort près du bord du ſoleil, ſon mouvement ſe faiſait très obliquement à ce bord, et agrandiſſait beaucoup l'effet d'une atmoſphère. En conſéquence je me ſuis ſingulièrement appliqué à obſerver ce paſſage et principalement les quatre contacts avec la plus grande attention, afin de me procurer des obſervations ſuffiſamment exactes pour pouvoir m'en ſervir avec avantage à determiner l'effet de cette atmo-

ſphère, ou du moins à m'aſſurer de ſon exiſtence. Je puis dire maintenant que les réſultats de mes calculs, de quelque manière que je les combine, en ſuppoſant l'obſervation et le diamètre de Mercure employé dans mes calculs rigoureuſement exacts, m'indiquent la préſence d'un effet ſemblable à celui d'une réfraction ou inflexion que ſouffriraient les rayons ſolaires dans leur paſſage auprès du globe de Mercure. Voici comment.

11. J'ai démontré dans le petit Traité que je viens de citer que la combinaiſon des deux obſervations des contacts extérieurs doit donner le même inſtant pour celui du milieu du paſſage que la combinaiſon des deux contacts intérieurs, et que cet inſtant du milieu du paſſage déduit de l'une et de l'autre combinaiſon reſtera toujours abſolument le même, qu'on ſuppoſe la planete entourée d'une atmoſphère ou non. Il eſt évident qu'à plus forte raiſon le milieu du paſſage déduit de la combinaiſon des deux obſervations du centre de la planète ſur la bord du ſoleil ne ſera point altéré par l'effet d'une atmoſphère, puiſqu'elle n'influe pas même ſur chacune de ces deux obſervations ſéparément. Enſuite j'ai encore fait voir que dans la ſuppoſition d'une atmoſphère autour de la planète qui paſſe ſur le diſque du ſoleil, le milieu du paſſage déduit de la combinaiſon de l'obſervation du contact extérieur de l'entrée avec celle du contact intérieur de la ſortie, doit arriver plus tard ; et le milieu du paſſage donné par la combinaiſon du contact intérieur de l'entrée avec le contact exterieur de la ſortie, doit arriver préciſément de la même quantité plutôt que le milieu du paſſage conclu par la combinaiſon des deux contacts intérieurs, ou par celle des deux contacts extérieurs, ou, ce qui revient encore au même, que le milieu du paſſage que donneraient indiſtinctement toutes les obſervations des contacts combinées comme on voudra, ſi la planète n'avait point d'atmoſphère. La différence

ou la quantité, dont le milieu du paſſage eſt trouvé plus tard ou plutôt, ſera l'effet de l'atmoſphère de la planète ſur le milieu du paſſage.

12. En conſéquence de ces principes j'ai donc fait encore deux combinaiſons pour en déduire le milieu du paſſage, et j'ai trouvé que la combinaiſon du contact extérieur de l'entrée avec le contact intérieur de la ſortie donne cet inſtant à 3h. 40′ 13″,6; celle du contact interieur de l'entrée avec le contact extérieur de la ſortie le donne à 3 h. 39′ 20″,8. Or on a vu (art. 7.) que le milieu du paſſage, ſelon la combinaiſon des deux contacts intérieurs et celle des deux contacts extérieurs eſt arrivé à 3 h. 39′ 47″,2, quantité qui ſe trouve entre les deux précédentes et exactement à égales diſtances de l'une et de l'autre, ſavoir de 26″,4. Il eſt donc évident que l'effet de l'atmoſphère de Mercure dans ce paſſage-ci a été 26″,4 de tems ſur le milieu du paſſage, en faiſant abſtraction de toute autre cauſe qui peut avoir quelque influence ſur les obſervations des contacts.

13. Mais ces 26″,4 ne peuvent provenir que de trois cauſes : ou de l'inexactitude des obſervations, ou d'une erreur ſur les diamètres du ſoleil et de Mercure employés dans les calculs, ou de la réfraction des rayons ſolaires dans l'atmoſphère de Mercure ; ainſi que je l'ai démontré dans mon petit Traité ſur les Paſſages de Venus et de Mercure, et où j'arrive, après un examen rigoureux de toutes les hypothèſes poſſibles, à cette équation générale $A = \alpha \pm \beta \pm \gamma + \varepsilon$; dans laquelle A eſt la quantité déterminée par les combinaiſons des obſervations, comme ici les 26″,4, et par conſéquent connue; α, β, γ, ε, la part qui en appartient reſpectivement à l'atmoſphère de la planète, à l'erreur de ſon diamètre, à celle du ſoleil et à l'erreur de l'obſervation. Je ferai remarquer ſeulement au ſujet de cette formule qu'il n'y a que l'erreur ſur le diamètre de la planète dont l'effet β pourrait quelquefois entrer

dans le valeur de A comme quantité négative, mais alors, loin de nuire à l'opinion d'attribuer cet effet, qui eſt ici de 26″,4, à l'atmoſphère de la planète, elle la favoriſerait plutôt. Quant à l'erreur ſur le diamètre du ſoleil, ſon influence peut être regardée comme nulle dans tous les cas, c'eſt à dire γ peut toujours être regardé comme zéro, à moins que l'erreur ſur le diamètre du ſoleil ne ſoit très conſidérable, et c'eſt un des avantages de ma méthode pour déterminer la valeur de A. Or la probabilité ſerait en faveur des obſervations, puiſqu'elles donnent, ce qui eſt conforme à la théorie, le même intervalle de tems entre les deux contacts de l'entrée qu'entre les deux contacts de la ſortie, ainſi l'on aurait ici $\varepsilon = 0$. Quant aux deux autres cauſes, il n'en eſt pas de même, puiſqu'il eſt évident par la formule générale qu'une même quantité conſidérée comme erreur ſur les diamètres, ou comme réfraction des rayons ſolaires dans l'atmoſphère de Mercure, eſt capable de produire exactement le même effet. Mais comme il eſt très probable que les trois cauſes à la fois peuvent avoir concouru à produire ces 26″,4 = A, et qu'il eſt abſolument impoſſible, d'après ma méthode comme d'après toute autre, de démêler les effets pour aſſigner à chaque cauſe la part qui lui appartient dans la valeur de A, le problème reſtera indéterminé à cet égard, par conſéquent ſi l'on ne veut admettre qu'une ſeule cauſe, on ſera libre de ſe décider pour l'une ou pour l'autre; or la queſtion n'étant plus alors qu'une affaire d'opinion, le choix doit tomber néceſſairement ſur la cauſe qui eſt la moins connue, et dont nous ne pouvons pas raiſonnablement conteſter l'exiſtence. On peut donc fort bien attribuer cet effet à l'atmoſphère de Mercure ſans craindre de ſe tromper beaucoup. Il s'en ſuit donc qu'en regardant ces 26″,4 ſimplement comme effet de l'atmoſphère de Mercure, la quantité, qui en réſulterait pour l'inflexion ou la réfraction réelle de cette atmoſphere, nous aſſurerait au moins d'une eſpece de limite qu'elle ne ſurpaſſerait jamais ou du moins très-rarement, puiſque l'inflexion des rayons ſolaires, à elle-ſeule, ne peut égaler la ſomme des trois cauſes dont elle fait partie, que dans l'hypothèſe particulière des deux autres égales à zéro. Cette manière d'enviſager le problème me donnera du moins une connaiſſance approchée de la valeur de la réfraction de l'atmoſphère de Mercure, dont je n'aurais ſans cette recherche abſolument aucune idée. Or il me ſemble qu'il vaut mieux acquérir une connaiſſance imparfaite que de reſter dans l'ignorance abſolue.

14. La quantité de cet effet, quel qu'il ſoit, étant donc connue, j'ai cherché à concilier les deux valeurs de la plus courte diſtance des centres trouvées par les contacts intérieurs et extérieurs, et pour cet effet je me ſuis propoſé ce problème qui doit s'en ſuivre naturellement, puiſque la valeur de γ peut toujours être regardée comme zéro: *Déterminer le diamètre du ſoleil, celui de Mercure étant connu par obſervation, tel que la durée donnée par les contacts extérieurs et la durée des contacts intérieurs faſſent trouver, l'une et l'autre, la même quantité pour valeur de la plus courte diſtance des centres.* Ce problème étant reſolu en nommant a, la demi-durée entre les contacts exterieurs, b celle des contacts intérieurs, δ le diametre de Mercure, x la différence des demi-diamètres de Mercure et du ſoleil, y la plus courte diſtance des centres cherchée, je trouve $x=\frac{a^2-b^2-\delta^2}{2\delta}$ et $y=\sqrt{\left(\frac{a^2-b^2-\delta^2}{2\delta}\right)^2-b^2}$; formules qui étant évaluées après avoir convenablement corrigé des 26,″4 chaque obſervation des quatre contacts, et augmenté la durée des contacts extérieurs de ces 6″ dont j'ai parlé ci-devant, me donnent $x=967'',04$ valeur plus

petite de o″,44 que celle que j'avais ſuppoſée dans mes calculs, et $y = 15'45'',24$ plus grande de o″,1 que celle qui m'a été donnée par la combinaiſon des deux contacts intérieurs; ainſi tous les réſultats trouvés par cette combinaiſon n'auront beſoin d'aucune correction, et je les adopterai, comme étant les meilleurs, tels qu'ils ſont rapportés dans la table ci-deſſus art. 7.

15. En ſuppoſant donc que les 26″,4 ſoient produites par l'atmoſphère de Mercure, je trouve o″276 pour la réfraction horizontale de cette atmoſphère. Les obſervations du paſſage de Venus en 1769 m'ayant annoncé un effet ſemblable d'environ 8″ à 9″ de tems, je trouve ſa réfraction horizontale d'environ o″205 qui n'eſt qu'à peu près les deux tiers de celle de Mercure.

CONCLUSION.

16. Quelque peu de confiance que j'attache à ces réſultats, et quelque ſoit l'opinion que j'adopte pour choiſir entre les cauſes qui peuvent produire l'effet en queſtion, je crois du moins pouvoir conclure avec certitude, ce que je m'étais principalement propoſé de prouver dans ce Mémoire, que les obſervations dont il s'agit ici, malgré le dégré d'incertitude qu'on puiſſe leur ſuppoſer, indiquent clairement l'exiſtence d'un effet ſemblable à celui d'une atmoſphère qui environnerait la planète; et que cet effet, ſoit qu'il provienne effectivement de cette atmoſphère, ou d'une erreur ſur le diamètre de la planète, ou d'une erreur dans les obſervations, ou qu'il ſoit le réſultat de l'action ſimultanée des trois cauſes réunies, il ſe fait ſentir evidemment dans toutes les obſervations des paſſages de Venus et de Mercure, du moins dans toutes celles que j'ai calculées. Par conſéquent l'influence de ces cauſes qui altèrent les obſervations d'une maniere ſi ſenſible me parait, ſous tous les points de vue, mériter l'attention des aſtronomes; et je ſuis très perſuadé que faute d'y avoir eu

égard dans la comparaiſon des obſervations du paſſage de Venus pour en déduire la parallaxe du ſoleil, bien des aſtronomes ſeraient dans le cas de recommencer leur calcul. Heureuſement je n'ai pas ce reproche à me faire ; car j'ai conſtamment évité avec le plus grand ſoin l'effet d'une atmoſphère autour de Venus en choiſiſſant les obſervations pour en faire la comparaiſon de manière que l'effet de cette atmoſphère, qu'il ait exiſté ou non, ſe trouvait toujours réduit à zéro. C'eſt ainſi que dans mon Mémoire ſur le paſſage de Venus en 1769 j'ai fixé à 8″,7 la parallaxe horizontale du ſoleil dans ſes moyennes diſtances à la terre.

XXV. *Thoughts on the conſtituent Parts of Water and of Dephlogiſticated Air; with an Account of ſome Experiments on that Subject. In a Letter from Mr.* James Watt, *Engineer, to Mr.* De Luc, *F. R. S.*

Read April 29, 1784.

DEAR SIR,

Birmingham,
November 26, 1783.

IN compliance with your deſire, I ſend you an account of the hypotheſis I have ventured to form on the probable cauſes of the production of water from the deflagration of a mixture of dephlogiſticated and inflammable airs, in ſome of our friend Dr. PRIESTLEY's experiments.

I feel much reluctance to lay my thoughts on theſe ſubjects before the public in their preſent indigeſted ſtate, and without having been able to bring them to the teſt of ſuch experiments as would confirm or refute them; and ſhould, therefore, have delayed the publication of them until theſe experiments had been made, if you, Sir, and ſome other of my philoſophical friends, had not thought them as plauſible as any other conjectures which have been formed on the ſubject; and that though they ſhould not be verified by further experiments, or approved of by men of ſcience in general, they may perhaps merit a diſcuſſion, and give riſe to experiments which may throw light on ſo important a ſubject.

I firſt thought of this way of ſolving the phænomena in endeavouring to account for an experiment of Dr. PRIESTLEY's, ley's, wherein water appeared to be converted into air; and I communicated my ſentiments in a letter addreſſed to him, dated April 26, 1783*, with a requeſt that he would do me the honour to lay them before the Royal Society; but as, before he had an opportunity of doing me that favour, he found, in the proſecution of his experiments, that the apparent converſion of water into air, by expoſing it to heat in porous earthen veſſels, was not a real tranſmutation, but an exchange of the elaſtic fluid for the liquid, in ſome manner not yet accounted for; therefore, as my theory was no ways applicable to the explaining theſe experiments, I thought proper to delay its publication, that I might examine the ſubject more deliberately, which my other avocations have prevented me from doing to this time.

“1. It has been known for ſome time, that inflammable air contained much phlogiſton; and Dr. Priestley has found, by ſome experiments made lately, that it “ is either wholly “ pure phlogiſton, or at leaſt that it contains no apparent mixture of any other matter.” (In my opinion, however, it contains a ſmall quantity of water and much elementary heat.)

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* This letter Dr. Priestley received at London; and, after ſhewing it to ſeveral Members of the Royal Society, he delivered it to Sir Joseph Banks, the Preſident, with a requeſt that it might be read at ſome of the public meetings of the Society; but before that could be complied with, the author, having heard of Dr. Priestley's new experiments, begged that the reading might be delayed. The letter, therefore, was reſerved until the end of April laſt; when, at the author's requeſt, it was read before the Society. It has been judged unneceſſary to print that letter, as the eſſential parts of it are repeated, almoſt *verbatim*, in this letter to M. De Luc; but, to authenticate the date of the author's ideas, the parts of it which are contained in the preſent letter are marked with double commas.

heat *.) "He found, that by expoſing the calces of metals "to the ſolar rays, concentrated by a lens, in a veſſel containing "inflammable air only, the calces of the ſofter metals "were reduced to their metallic ſtate;" and that the inflammable air was abſorbed in proportion as they became phlogiſticated; and, by continually ſupplying the veſſel with inflammable air, as it was abſorbed, he found, that out of 101 ounce meaſures, which he had put into the veſſel, 99 ounce meaſures were abſorbed by the calces, and only two ounce meaſures remained, which, upon examination, he found to be nearly of the ſame quality the whole quantity had been of before the experiment, and to be ſtill capable of deflagrating in conjunction with atmoſpheric or with dephlogiſticated air. *Therefore, as ſo great a quantity of inflammable air had been abſorbed by the metallic calces; the effect of reducing them to their metallic ſtate had been produced; and the ſmall remaining portion was ſtill unchanged, at leaſt had ſuffered no change which might not be attributed to its original want of purity; it was reaſonable to conclude, that inflammable air muſt be the pure phlogiſton, or the matter which reduced the calces to metals.*

2. "The ſame ingenious philoſopher mixed together certain "proportions of pure dry dephlogiſticated air and of pure "dry inflammable air in a ſtrong glaſs veſſel, cloſely ſhut, "and then ſet them on fire by means of the electric ſpark," in the ſame manner as is done in the inflammable air piſtol. "The firſt effect was the appearance of red heat or inflamma-

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* Previous to Dr. PRIESTLEY's making theſe experiments, M. KIRWAN had proved, by very ingenious deductions from other facts, that inflammable air was, in all probability, the real phlogiſton, in an aerial form. Theſe arguments were perfectly convincing to me; but it ſeems more proper to reſt that part of the preſent hypotheſis on the direct experiment.

“ tion in the airs, which was ſoon followed by the glaſs veſſel becoming hot. The heat gradually pervaded the glaſs, and was diſſipated in the circumambient air, and as the glaſs grew cool, a miſt or viſible vapour appeared in it, which was condenſed on the glaſs in the form of moiſture or dew *. When the glaſs was cooled to the temperature of the atmoſphere, if the veſſel was opened with its mouth immerſed in water or mercury, ſo much of theſe liquids entered, as was ſufficient to fill the glaſs within about $\frac{1}{100}$dth part of its whole contents; and this ſmall reſiduum may ſafely be concluded to have been occaſioned by ſome impurity in one or both kinds of air. The moiſture adhering to the glaſs, after theſe deflagrations, being wiped off, or ſucked up, by a ſmall piece of ſponge paper, firſt carefully weighed, was found to be exactly, or very nearly, equal in weight to the airs employed.”

“ In ſome experiments, but not in all, a ſmall quantity of a ſooty-like matter was found adhering to the inſide of the glaſs,” the origin of which is not yet inveſtigated; but Dr. Priestley thinks, that it ariſes from ſome minute grains of the mercury that was uſed in order to fill the glaſs with the air, which being ſuper-phlogiſticated by the inflammable air, aſſumed that appearance; but, from whatever cauſe it proceeded, “ the whole quantity of ſooty-like matter was too ſmall to be an object of conſideration, particularly as it did not occur in all the experiments.”

I am obliged to your friendſhip for the account of the experiments which have been lately made at Paris on this ſubject,

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* I believe that Mr. Cavendish was the firſt who diſcovered that the combuſtion of dephlogiſticated and inflammable air produced moiſture on the ſides of the glaſs in which they were fired.

with large quantities of theſe two kinds of air, by which the eſſential point ſeems to be clearly proved, that the deflagration or union of dephlogiſticated and inflammable air, by means of ignition, produces a quantity of water equal in weight to the airs; and that the water, thus produced, appeared, by every teſt, to be pure water. As I am not furniſhed with any particulars of the manner of making the experiment, I can make no obſervations on it, only that, from the character you give me of the gentlemen who made it, there is no reaſon to doubt of its being made with all neceſſary precautions and accuracy, which was farther ſecured by the large quantities of the two airs conſumed.

3. " Let us now conſider what obviouſly happens in the " caſe of the deflagration of the inflammable and dephlogiſti- " cated air. Theſe two kinds of air unite with violence, they " become red-hot, and upon cooling totally diſappear. When " the veſſel is cooled, a quantity of water is found in it equal " to the weight of the air employed. This water is then the " only remaining product of the proceſs, and *water*, *light*, and " *heat*, are all the products," unleſs there be ſome other matter ſet free which eſcapes our ſenſes.

" *Are we not then authoriſed to conclude, that water is com-* " *poſed of dephlogiſticated air and phlogiſton, deprived of part of* " *their latent or elementary heat; that dephlogiſticated or pure air* " *is compoſed of water deprived of its phlogiſton, and united to* " *elementary heat and light; and that the latter are contained in* " *it in a latent ſtate, ſo as not to be ſenſible to the thermometer or* " *to the eye; and if light be only a modification of heat, or a cir-* " *cumſtance attending it, or a component part of the inflammable* " *air, then pure or dephlogiſticated air is compoſed of water de-* " *prived of its phlogiſton and united to elementary heat?*"

4.

4. "It appears, that dephlogiſticated water," or, which may be a better name for the baſis of water and air, the element you call *humor*, "has a more powerful attraction for "phlogiſton than it has for latent heat, but that it cannot "unite with it, at leaſt not to the point of ſaturation, or to "the total expulſion of the heat, unleſs it be firſt made red-hot," or nearly ſo. "The electric ſpark heats a portion o "it red-hot, the attraction between the humor and the phlogiſton takes place, and the heat which is let looſe from this "firſt portion heats a ſecond, which operates in a like manner "on the adjoining particles, and ſo continually until the whole "is heated red-hot and decompoſed." Why this attraction does not take place to the ſame degree in the common temperature of the atmoſphere, is a queſtion I am not yet able to ſolve; but it appears, that, in ſome circumſtances, "dephlogiſticated air can unite, in certain degrees, with phlogiſton "without being changed into water." Thus Dr. PRIESTLEY has found, that by taking clean filings of iron, which, alone, produce only inflammable air of the pureſt kind, and *mercurius calcinatus per ſe*, which gives only the pureſt dephlogiſticated air, and expoſing them to heat, in the ſame veſſel, he obtained neither dephlogiſticated nor inflammable air, "but in their "place fixed air." Yet it is well known, that a mixture of dephlogiſticated and inflammable air will remain for years in cloſe veſſels in the common heat of the atmoſphere, without ſuffering any change, the mixture being as capable of deflagration at the end of that time as it was when firſt ſhut up. Theſe facts the Doctor accounts for, by ſuppoſing that the two kinds of air, when formed at the ſame time in the ſame veſſel, can unite in their *naſcent* ſtate; but that, when fully formed, they are incapable of acting upon one another, unleſs they are

firſt ſet in motion by external heat. "Phlogiſticated air ſeems "alſo to be another compoſition of phlogiſton and dephlogiſticated air;" but in what proportions they are united, or by what means, is ſtill unknown. It appears to me to be very probable, that fixed air contains a greater quantity of phlogiſton than phlogiſticated air does, becauſe it has a greater ſpecific gravity, and becauſe it has more affinity with water.

5. "For many years I have entertained an opinion, that air "was a modification of water, which was originally founded "on the facts that in moſt caſes, wherein air was actually "made," which ſhould be diſtinguiſhed from thoſe wherein it is only extricated from ſubſtances containing it in their pores, or otherwiſe united to them in the ſtate of air, "the ſubſtances were ſuch as were known to contain water as one of "their conſtituent parts, yet no water was obtained in the "proceſſes," except what was known to be only looſely connected with them, ſuch as the water of the cryſtallization of ſalts. "This opinion aroſe from a diſcovery," that the latent heat contained in ſteam diminiſhed in proportion as the ſenſible heat of the water from which it was produced increaſed; or, in other words, "that the latent heat of ſteam was leſs "when it was produced under a greater preſſure, or in a more "denſe ſtate, and greater when it was produced under a leſs "preſſure, or in a leſs denſe ſtate; which led me to conclude, "that when a very great degree of heat was neceſſary for the "production of the ſteam, the latent heat would be wholly "changed into ſenſible heat; and that, in ſuch caſes, the "ſteam itſelf might ſuffer ſome remarkable change. I now "abandon this opinion in ſo far as relates to the change of "water into air, as I think that may be accounted for on better "principles."

6. "In every caſe, wherein dephlogiſticated air has been "produced, ſubſtances have been employed, ſome of whoſe "conſtituent parts have a ſtrong attraction for phlogiſton, and, "as it would appear, a ſtronger attraction for that ſubſtance "than *humor* has; they ſhould, therefore, dephlogiſticate the "water" or fixed air, and the *humor* thus ſet free ſhould unite to the matter of fire and light and become pure air. Dephlogiſticated air is produced in great abundance from melted nitre. "The acid of nitre has a greater attraction for phlogiſton than "any other ſubſtance is known to have; and it is alſo certain, "that nitre, beſides its water of cryſtallization, contains a "quantity of water as one of its elementary parts, which "water adheres to the other parts of the nitre with a force "ſufficient to enable it to ſuſtain a red heat. When the nitre "is melted, or made red-hot, the acid acts upon the water and "dephlogiſticates it; and the fire ſupplies the *humor* with the "due quantity of heat to conſtitute it air, under which form "it immediately iſſues. It is not eaſy to tell what becomes of "the acid of nitre and phlogiſton, which are ſuppoſed to be "united," as they ſeem to be loſt in the proceſs. Dr. Priestley has lately made ſome experiments, with a view to aſcertain this point. He diſtilled dephlogiſticated air from pure nitre, in an earthen retort glazed within and without. He employed 2 oz. = 960 grains of nitre: the retort was placed in an air furnace, and, by means of an intenſe heat, he obtained from the nitre in one experiment 787, and in another experiment 800 ounce measures of dephlogiſticated air; and he found that, upon weighing the retort and nitre before and after the proceſs, they had ſuffered a loſs of weight equal to the weight of the air, and to the water of cryſtallization of the nitre; but nothing more. He remarked, that the air had a pungent

ſmell, which he could not diveſt it of by waſhing; and that the water in which the air was received had become ſlightly acid. I examined a portion of this water, which he was ſo kind as to ſend me, and found by it that the whole of the receiving water had contained the acid belonging to 2 drams = 120 grains of nitre. I alſo examined the reſiduum and the retort in which the diſtillation had been performed, and found the reſiduum highly alkaline, yet containing a minute quantity of phlogiſticated nitrous acid. It had acted conſiderably upon the retort, and had diſſolved a part of it, which was depoſited in the form of a browniſh powder, when the ſaline part was diſſolved in water. This earthy powder I have not yet thoroughly examined, but have no doubt that it principally conſiſts of the earth of the retort. This experiment, and all others tried in earthen veſſels, leave us ſtill at a loſs to determine what becomes of the acid and phlogiſton. They ſeem either to remain mixed with the air, in the form of an incoercible gas; or to unite with the alkali, or with the earth of the retort, in ſome manner ſo as not to be eaſily ſeparated from them; or elſe they are imbibed by the retorts themſelves, which are ſufficiently porous to admit of ſuch a ſuppoſition.

All that appears to be concluſive from this experiment is, that above one half of the weight of the nitre was obtained in the form of dephlogiſticated air; and that the reſiduum ſtill contained ſome nitrous acid united to phlogiſton.

7. Finding that the action of the nitre on the retort tended to prevent any accurate examination of the products, I had recourſe to combinations of the nitrous acid with earths from which the dephlogiſticated air is obtained with leſs heat than from nitre itſelf. As theſe proceſſes have been particularly deſcribed by Dr. Priestley, by Mr. Scheele, and others, I

ſhall not enter into any detail of them; but ſhall mention the general phænomena which I obſerved, and which relate to the preſent ſubject.

The earths I uſed were magneſia alba, calcareous earth, and minium or the red calx of lead. I diſſolved them in the reſpective experiments in nitrous acid dephlogiſticated by boiling, and diluted with proper proportions of water. I made uſe of glaſs retorts, coated with clay; and I received the air in glaſs veſſels, whoſe mouths were immerſed in a glazed earthen baſon, containing the ſmalleſt quantity of water that could be uſed for the purpoſe. As ſoon as the retort was heated a little above the heat of boiling water, the ſolutions began to diſtil watery vapours containing nitrous acid. Soon after theſe vapours ceaſed, yellow fumes, and in ſome of the caſes dark red fumes, began to appear in the neck of the retort; and at the ſame time there was a production of dephlogiſticated air, which was greater in quantity from ſome of theſe mixtures than from others, but continued in all of them until the ſubſtances were reduced to dryneſs. I found, in the receiving water &c. very nearly the whole of the nitrous acid uſed for their ſolution, but highly phlogiſticated, ſo as to emit nitrous air by the application of heat; and there is reaſon to believe, that with more precaution the whole might have been obtained.

8. As the quantity of dephlogiſticated air produced by theſe proceſſes did not form a ſufficient part of the whole weight, to enable me to judge whether any of the real acid entered into the compoſition of the air obtained, I ceaſed to purſue them further, having learned from them the fact, *that however much the acid and the earths were dephlogiſticated before the ſolution, the acid always became highly phlogiſticated in the proceſs*.

In order to examine whether this phlogiſton was furniſhed by the earths, ſome dephlogiſticated nitrous acid was diſtilled from minium till no more acid or air came over. More of the ſame acid was added to the minium as ſoon as it was cold, and the diſtillation repeated, which produced the ſame appearance of red fumes and dephlogiſticated air. This operation was repeated a third time on the ſame minium, without any ſenſible variation in the phænomena. The proceſs ſhould have been ſtill farther repeated, but the retort broke about the end of the third diſtillation. The quantity of minium uſed was 120 grains, and the quantity of nitrous acid added each time was 240 grains, of ſuch ſtrength that it could diſſolve half its weight of mercury, by means of heat.

It appears from this experiment, that unleſs minium be ſuppoſed to conſiſt principally of phlogiſton, the ſource of the phlogiſton, thus obtained, was either the nitrous acid itſelf, or the water with which it was diluted; or elſe that it came through the retort with the light, for the retort was in this caſe red-hot before any air was produced; yet this latter concluſion does not appear very ſatiſfactory, when it is conſidered, that in the proceſs wherein the earth made uſe of was magneſia, the retort was not red-hot, or very obſcurely ſo, in any part of the proceſs; and by no means luminous, when the yellow and red fumes firſt made their appearance.

9. As the principal point in view was to determine whether any part of the acid entered into the compoſition of the air, I reſolved to employ ſome ſubſtance which would part with the acid in a moderate heat, and alſo give larger quantities of air than had been obtained in the former proceſſes. Mercury was thought a proper ſubſtance for this purpoſe. 240 grains of mercury were put into a glaſs retort with 480 grains

of diluted dephlogiſticated nitrous acid, which was the quantity neceſſary to diſſolve the whole of the mercury, a gentle heat was applied, and as ſoon as the common air contained in the retort was diſſipated, a veſſel was placed to receive the nitrous air proceeding from the ſolution, which was 16 ounce meaſures. When it had ceaſed to give nitrous air, the neck of the retort became hot from the watery ſteams of the acid. The air receiver was taken away, and a common receiver was luted on, with a little water in it, to condenſe the vapours, and a quantity of dilute, but highly phlogiſticated, acid was caught in the receiver. When the watery vapours had nearly come over, and yellow fumes appeared in the neck of the retort, the common receiver was removed, and the air receiver replaced; about four ounces of very ſtrong nitrous air paſſed up immediately, the fumes in the retort became red, and dephlogiſticated air paſſed up, which, uniting with the nitrous air in the receiver, produced red fumes in the receiver; and the two kinds of air acting upon one another, their bulk was reduced to half of an ounce meaſure. At this period the fumes in the retort were of a dark red colour, and dephlogiſticated air was produced very faſt. After a ſhort time, ſome orange-coloured ſublimate appeared in the upper part of the retort, and extended a little way along its neck, the red colour of the fumes gradually diſappeared, and the neck of the retort became quite clear. At the ſame time that this happened, ſmall globules of mercury appeared in the neck of the retort, and accumulated there until they ran down in drops. The production of the air was now very rapid, and accompanied with much of the white cloud or powdery matter, which paſſed up with the air into the receiver, and mixed with the water, but did not diſſolve in it. After giving about 36 ounce meaſures of dephlogiſticated air, it ſuddenly ceaſed to give any more; and the retort being cooled, the bulb was found to be quite empty, excepting a ſmall quantity of black powder, which, on being rubbed on the hand, proved to be moſtly running mercury. The orange-coloured ſublimate was waſhed out of the neck of the retort, and what running mercury was in it was ſeparated, and added to that which had run down into the baſon among the water. The whole fluid mercury, when dried, weighed 218 grains; therefore 22 grains remained in the form of ſublimate, which, I believe, would alſo have been reduced if I could have applied heat in a proper manner to the neck of the retort, as ſome of it, to which heat could be applied, diſappeared.

10. The 16 ounce meaſures of nitrous air, which had been produced in the ſolution of the mercury, and had remained confined by water in the receiver, was converted into nitrous acid by the gradual admiſſion of common air, and was taken up by the water; this water was added to that in the baſon, which had ſerved to receive the dephlogiſticated air. The whole quantity was about two quarts, was very acid to the taſte, and ſparkling with nitrous air. It was immediately put into bottles, and well corked, until it had loſt the heat gained in the operation. In order to determine the quantity of acid in the receiving water and in the ſublimate, I diſſolved, firſt, alkali of tartar in water, and filtered the ſolution. 352 grains of this alkaline ſolution ſaturated 120 grains of the nitrous acid I had employed to diſſolve the mercury, and 1395 grains of the ſame alkaline ſolution ſaturated the orange-coloured precipitate, and all the acid liquor obtained from the proceſs: therefore we have the proportion as 352 : 120 :: 1395 : 475, from which it appears, that all the acid employed was recovered again in the form of acid, excepting only five grains;

a ſmaller quantity than what might reaſonably be ſuppoſed to be loſt in the proceſs by the extreme volatility of the nitrous air. In order to aſcertain the exact point of ſaturation, ſlips of paper, ſtained by the juice of the petals of the ſcarlet roſe, were employed, which were the niceſt teſt I could procure, as litmus will not ſhew the point of ſaturation of any liquor containing much phlogiſticated nitrous acid, or even fixed air, but will turn red, and ſhew it to be acid, when the teſt of thoſe leaves, violets, and ſome other of the like kind, will turn green in the ſame liquor, and ſhew it to be alkaline. But the exact point of ſaturation of ſo dilute a liquor is ſo very difficult to aſcertain, than an error might eaſily be committed, notwithſtanding the attention beſtowed upon it. Suppoſing this experiment to be unexceptionable, the concluſions which may be drawn from it are very favourable to the hypotheſis I endeavour to ſupport. *Thirty-ſix ounce meaſures of dephlogiſticated air were obtained, and only five grains of a weak nitrous acid were loſt in the proceſs. Two hundred and eighteen grains of mercury out of two hundred and forty were revived, and all the dephlogiſticated nitrous acid employed is found to be highly phlogiſticated in the proceſs. It appears, that the nitrous acid does not enter into the compoſition of dephlogiſticated air; it ſeems only to ſerve to abſorb phlogiſton from the watery part of the mercurial nitre.*

11. As this laſt proceſs proved very tedious and complicated on account of the neceſſity of aſcertaining the quantity of acid in the receiving water, by means of an alkali which afforded a double ſource of error in the point of ſaturation, I reſolved to try the diſtillation of dephlogiſticated air from cubic nitre in a glaſs veſſel, and to draw from it only ſuch a quantity of air as it would yield without acting much upon the retort, which

latter circumſtance is eſſentially neceſſary to be attended to. An ounce of the cryſtals of mineral alkali were diſſolved in nitrous acid, and the mixture brought to an exact ſaturation by the teſt of litmus; 30 ounce meaſures of air were diſtilled from it; which, during the latter part of the proceſs, was accompanied with ſlightly yellow fumes; the receiving water was found to be acid, and the reſiduum alkaline. The reſiduum being diſſolved in the receiving water, the ſolution was neutral, or very nearly ſo, by every teſt; for in this caſe litmus might be uſed, as the acid was very ſlightly phlogiſticated. On adding a few drops of a very dilute nitrous acid, the teſts ſhewed the liquor to be acid.

12. Encouraged by the ſucceſs of this experiment, I took an ounce = 480 grains of pure common nitre, and put it into a flint-glaſs retort, coated, which was placed in a furnace. It began to give air about the time it became red-hot, and during the latter part of the proceſs this air was accompanied with yellowiſh fumes. I ſtopped the proceſs when it had produced 50 ounce meaſures of air. The receiving water, and particularly the air, had a ſtrong but peculiar ſmell of nitrous acid. The air was well waſhed with the receiving water, but was not freed from the ſmell. The receiving water, which was 50 ounces, was ſlightly acid, and the reſiduum alkaline. I diſſolved the latter in the former, and found the mixture alkaline. 10 grains of weak nitrous acid were added to it, which ſaturated it, and 105 grains of this ſpirit of nitre was found to contain the acid of 60 grains of nitre; therefore the 10 grains contained the acid of 5,7 grains of nitre, which, by Mr. KIRWAN's experiments is equal to two grains of real nitrous acid. *We have, therefore, 34 grains weight of dephlogiſticated air produced, and only two grains of real acid miſſing*; and it is not

certain that this quantity was deſtroyed, becauſe ſome portion of the glaſs of the retort was diſſolved by the nitre, and ſome part of the materials employed in making the glaſs being alkali, we may conclude, that the alkali of the nitre would be augmented by the alkali of that part of the glaſs it had diſſolved. As the glaſs cracked into ſmall pieces on cooling, and ſome part of the coating adhered firmly to it, the quantity of the glaſs that was diſſolved could not be aſcertained. *From this experiment it appears, that if any of the acid of the nitre enters into the compoſition of the dephlogiſticated air, it is a very ſmall part; and it rather ſeems, that the acid, or part of it, unites itſelf ſo firmly to the phlogiſton as to loſe its attraction for water.*

13. "The vitriolic ſalts alſo yield dephlogiſticated air by "heat; and in theſe caſes the dephlogiſticated air is always "attended with a large quantity of vitriolic acid air or ſul"phureous vapour," even when the ſalts uſed are not known to contain any phlogiſtic matter. Mr. SCHEELE mentions his having obtained dephlogiſticated air from manganeſe diſſolved in acid of phoſphorus, and alſo from the arſenical acid: from whence it appears, that theſe acids, or perhaps any acid which can bear a red heat, can concur to the production of dephlogiſticated air. *It is neceſſary to remark, that no experiments have been yet publiſhed ſhewing that dephlogiſticated air can be produced from ſalts formed by the muriatic acid. The acids which produce ſalts ſuitable for this purpoſe, have all a ſtrong affinity with phlogiſton; and the marine acid has either a very ſmall affinity with it, or elſe is already ſaturated with it, at leaſt ſo far ſaturated as not to be able to attract it from the* humor.

14. "The dephlogiſticated air obtained from the pure calces "of metals may be attributed to the calces themſelves, attract"ing the phlogiſton from water which they have imbibed from

"the atmoſphere, or from dephlogiſticating the fixed air which "they are known to contain."

It is very probable, that the dephlogiſticated air extruded from growing vegetables may be owing to their dephlogiſticating the water they grow in; but it appears more probable, that the plants have a power of dephlogiſticating the fixed, or phlogiſticated, air of the atmoſphere.

"When dephlogiſticated and nitrous air are mixed, the de-"phlogiſticated air ſeizes part of the phlogiſton of the nitrous "air." The water contained in the nitrous air, and the other part of the phlogiſton, unite with the nitrous acid, which then aſſumes a liquid form, or at leaſt that of a denſe vapour; "and "that part of the latent heat of the two airs not eſſential to the "new combination is ſet at liberty*."

In the combuſtion of ſulphur the ſame thing happens, but in a greater degree; for the vitriolic acid, having a much weaker attraction for phlogiſton than air has, abandons it almoſt entirely to the latter, which is thereby converted into water, and in that form attracts the vitriolic acid, and reduces it to a liquid ſtate. The ſame reaſoning may be applied to the combuſtion of phoſphorus, which is attended with ſimilar effects.

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* I cannot take upon me to determine, from any facts which have come to my knowledge, whether any part of the dephlogiſticated air employed in this experiment is turned into fixed air; but I am rather inclined to think that ſome part is, becauſe the quantity of heat, which is ſeparated by the union of the two airs, does not ſeem to be ſo great as that which is ſeparated when the dephlogiſticated air is wholly changed into water: yet ſome water appears to be formed, becauſe when the mixture is made over mercury, the ſolution of the mercury in the nitrous acid aſſumes a cryſtallized form, which, however, may be due to the watery part of the nitrous air.

15. I ſhall not make, at preſent, any further deductions from what I myſelf conſider ſtill in the light of a conjectural hypotheſis, which I have perhaps dwelt upon too long already. I ſhall only beg your attention to ſome general reaſoning on the ſubject; which, however, may poſſibly ſerve more to ſhew the uncertainty of other ſyſtems on the conſtituent parts of air, than the certainty of this. Some of thoſe ſyſtems ſuppoſe dephlogiſticated air to be compoſed of an acid and ſomething elſe, ſome ſay phlogiſton. If an acid enters into the compoſition of it, why does not that acid appear again when the air is decompoſed, by means of inflammable air and heat? And why is the water which is the product of this proceſs pure water? And if an acid forms one of its conſtituent parts, why has nobody been able to detect any difference in the dephlogiſticated air, made by the help of different acids, when compared with one another, or with the air extruded by vegetables? Theſe airs, of ſuch different origins, appear to be exactly the ſame. And if phlogiſton be a conſtituent part of air, why does it attract phlogiſton with ſuch avidity? Some have, on the other hand, contended that air is compoſed of earth, united to acids or phlogiſton, or to both, or to ſome other matter. Here we muſt aſk, what earth it is which is one of the component parts of air? All earths which will unite with the nitrous or vitriolic acids, and with ſome others, ſuch as the phoſphoric and the arſenical acids, will ſerve as baſes for the formation of air, and the air produced from all of them appears by every teſt to be the ſame, when freed from accidental impurities. To this argument it is anſwered, that it is not any particular ſpecies of earth which is the baſis of air, but elementary or ſimple earth, which is contained in all of them. If this were the

matter of fact, would not that earth be found after the decompoſition of the air?

Mr. **Scheele** has formed an hypotheſis on this ſubject, in which he ſuppoſes heat to be compoſed of dephlogiſticated air united to phlogiſton, and that this combination is ſufficiently ſubtile to paſs through glaſs veſſels. He affirms, that the nitrous and other acids, when in an ignited ſtate, attract the phlogiſton from the heat, and ſet the dephlogiſticated air at liberty; but he does not ſeem to have been more ſucceſsful than myſelf in explaining what becomes of the acid of nitre and phlogiſton in the caſe of the decompoſition of nitre by heat. And ſince we know, from the late experiments, that water is a compoſition of air, or more properly, *humor* and phlogiſton, his whole theory muſt fall to the ground, unleſs that fact be otherwiſe accounted for, which it does not ſeem eaſy to do.

16. To return to the experiment of the deflagration of dephlogiſticated and inflammable air, "it appears from the "two airs becoming red-hot on their union, that the quantity "of heat contained in one or both of them, is much greater "than that contained in ſteam; becauſe, for the firſt moments "after the exploſion, the water depoſited by the air remains in "the form of ſteam, and conſequently retains the latent heat "due to that modification of water. This matter may be eaſily "examined by firing the mixture of dephlogiſticated and inflammable air in a veſſel immerſed in another veſſel containing a "given quantity of water of a known heat, and after the veſſel "in which the deflagration is performed is come to the ſame "temperature with the water in which it is immerſed, by examining how much heat that water has gained, which being "divided by the quantity of water produced by the decompoſition of the airs, will give the whole quantity of elemen-

"tary or latent heat which that water had contained, both as "air and as ſteam; and if from that quantity we deduct the "latent heat of the ſteam, the remainder will be the latent or "elementary heat contained more in air than in ſteam." This experiment may be made more compleatly by means of the excellent apparatus which Meſſ. LAVOISIER and DE LA PLACE have contrived for ſimilar purpoſes.

Until direct experiments are made, we may conclude, from thoſe which have been made by the gentlemen juſt named, on the decompoſitions of air by burning phoſphorus and charcoal, that the heat extricated during the combuſtion of inflammable and dephlogiſticated air is much greater than it appears to be; for they found that one Paris ounce (= 576 Pariſian grains) of dephlogiſticated air, when decompoſed by burning phoſphorus, melted 68,634 ounces of ice; and as, according to another of their experiments, ice, upon being melted, abſorbs 135° of heat, by FAHRENHEIT's ſcale, each ounce of air gave out, 68,634 × 135° = 9265°,590; that is to ſay, a quantity of heat which would have heated an ounce of water, or any other matter which has the ſame capacity for receiving heat as water has, from 32° to 9265½°: a ſurpriſing quantity! (It is to be underſtood, that all the latent heats mentioned herein are compared with the capacity of water). And when an ounce of dephlogiſticated air was changed into fixed air, by burning charcoal, or by the breathing of animals, it melted 29,547 oz. of ice; conſequently we have 29,547 × 135° = 3988°,845. the quantity of heat which an ounce of dephlogiſticated air loſes when it is changed into fixed air. By the heat extricated during the detonation of one ounce of nitre with one ounce of ſulphur, 32 ounces of ice were melted; and, by the experiment I have mentioned of Dr. PRIESTLEY's (6), it appears that nitre can produce one half of its weight of dephlogiſticated air. When the nitre and ſulphur are kindled, the dephlogiſticated air of the nitre unites with the phlogiſton of the ſulphur, and ſets its acid free, which immediately unites to the alkali of the nitre, and produces vitriolated tartar. The dephlogiſticated air, united to the phlogiſton, is turned into water, part of which is abſorbed by the vitriolated tartar, and part is diſſipated in the form of vapours, or unites to the nitrous air, or other air, produced in the proceſs.

As half an ounce of dephlogiſticated air is, in this proceſs, united by inflammation to a quantity of phlogiſton ſufficient to ſaturate it, and no fixed air is produced, it ſhould melt a quantity of ice equal to the half of what was melted by the combination of an ounce of air with phlogiſton in burning phoſphorus; that is, it ſhould melt 34,317 ounces of ice; and we find, by Meſſ. Lavoisier and De la Place's experiment, that it actually melted 32 ounces of ice: the ſmall difference may be accounted for by ſuppoſing, that the heat produced by the combuſtion might not be quite ſo great as that Dr. Priestley employed in his experiment; or that the nitre might be leſs pure, and conſequently not ſo much air formed. The two facts, however, agree near enough to permit us to conclude, *that dephlogiſticated air, in uniting to the phlogiſton of ſulphur, produces as much heat as it does in uniting with the phlogiſton of phoſphorus.*

17. According to Dr. Priestley's experiments, dephlogiſticated air unites compleatly with about twice its bulk of the inflammable air from metals. The inflammable air being ſuppoſed to be wholly phlogiſton, and being $\frac{1}{9,6}$ of the weight of an equal bulk of dephlogiſticated air, and being double in quantity, will be $\frac{1}{4,8}$ of the weight of the dephlogiſticated air

it unites with. Therefore one ounce (576 grains) of dephlogiſticated air, will require 120 grains of inflammable air, or phlogiſton, to convert it into water. And ſuppoſing the heat extricated by the union of dephlogiſticated and inflammable air to be equal to that extricated by the burning of phoſphorus, we ſhall find, that the union of 120 grains of inflammable air with 576 grains of dephlogiſticated air, extricates 9265° of heat.

18. In the experiment on the deflagration of nitre with charcoal, by Meſſ. Lavoisier and De la Place, an ounce of nitre and one third of an ounce of charcoal melted twelve ounces of ice. Suppoſing the ounce of nitre to have produced half an ounce of dephlogiſticated air, it ought to have conſumed 0,1507 ounces of charcoal, and ſhould have melted 14,773 ounces of ice; and I ſuppoſe it fell ſhort of its effect by the heat not being ſufficiently intenſe to decompoſe the nitre perfectly.

19. By the above gentlemen's experiment an ounce of charcoal required for its combuſtion 3,3167 ounces of dephlogiſticated air, and produced 3,6715 ounces of fixed air; therefore there was united to each ounce of air, when changed into fixed air, 61,5 grains of phlogiſton, and 3988° of heat were extracted. *It appears by theſe facts, that the union of phlogiſton, in different proportions, with dephlogiſticated air, does not extricate proportional quantities of heat.* For the addition of 61,5 grains produces 3988°. and the union of 120 grains produces 9265°. This difference may ariſe from a miſtake in ſuppoſing the ſpecific gravity of the inflammable air Dr. Priestley employed to have been only $\frac{1}{9,6}$ of that of dephlogiſticated air; for if it be ſuppoſed that its ſpecific gravity was a little more than $\frac{1}{8}$ of that of the dephlogiſticated air, then equal additions of phlogiſton would

have produced equal quantities of heat[*]: this matter ſhould therefore be put to the teſt of experiment, by deflagrating dephlogiſticated air with inflammable air of a known ſpecific gravity, or by finding how much dephlogiſticated air is neceſſary for the combuſtion of an ounce of ſulphur, the quantity of phlogiſton in which has been accurately determined by Mr. KIRWAN; or by finding the quantity of phlogiſton in phoſphorus, the quantity of dephlogiſticated air neceſſary for its decompoſition being known from Meſſ. LAVOISIER and DE LA PLACE's experiments.

On conſidering theſe latter gentlemen's experiments on the combuſtion of charcoal, a difficulty ariſes, to know what became of the remainder of the ounce of charcoal; for the dephlogiſticated air, in becoming fixed air, gained only the weight of 0,3548, or about $\frac{1}{3}$ of an ounce; about $\frac{2}{3}$ of an ounce are therefore unaccounted for. The weight of the aſhes of an ounce of charcoal is very inconſiderable; and, by ſome experiments of Dr. PRIESTLEY's, charcoal, when freed from fixed air, and other air which it imbibes from the atmoſphere, is almoſt wholly convertible into phlogiſton. The cauſe of this apparent loſs of matter, I doubt not, theſe gentlemen can explain ſatisfactorily, and very probably in ſuch a manner as will throw other lights on the ſubject.

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[*] Or it may ariſe from my being miſtaken, in ſuppoſing that the ſame quantity of heat is diſengaged by the union of dephlogiſticated air with phlogiſton, in the form of inflammable air, as is by its union with the phlogiſton of phoſphorus or ſulphur; and there appears to be ſome reaſon why there ſhould not; becauſe in theſe latter caſes the water, being united to the acids, cannot retain ſo much elementary heat as it can do when left in the form of pure water, which is the caſe when the inflammable air is uſed.

It is alſo worthy of enquiry, whether all the amazing quantity of heat let looſe in theſe experiments was contained in the dephlogiſticated air; or whether the greateſt portion of it was not contained in the phlogiſton or inflammable air. If it was all contained in the dephlogiſticated air, "*the general rule is not* "*fact, that elaſtic fluids are enlarged in their dimenſions in propor*" "*tion to the quantity of heat they contain*;" becauſe then, inflammable air, which is ten times the bulk of dephlogiſticated air, muſt be ſuppoſed to contain no heat at all; "and it is known, " from ſome experiments of my friend Dr. BLACK's, and ſome " of my own, that the ſteam of boiling water, whoſe latent " and ſenſible heat are only 1100°, reckoning from 60°, or tem" perate, is more than twice the bulk of an equal weight of " dephlogiſticated air." It ſeems, however, reaſonable to ſuppoſe, that the greater quantity of heat ſhould be contained in the rarer fluid.

It may be alledged, that in proportion to the quantity of phlogiſton that is contained in any fluid, the quantity of heat is leſſened. But if we reaſon by analogy, the attraction of the particles of matter to one another in other caſes is increaſed by phlogiſton, and "bodies are thereby rendered ſpecifically " heavier;" and we know of no other ſubſtance beſides heat which can be ſuppoſed to ſeparate the particles of inflammable air, and to endow it with ſo very great an elaſtic power, and ſo ſmall a ſpecific gravity. On the other hand, if a great quantity of elementary heat be allowed to be contained in inflammable air, on account of its bulk, the ſame reaſoning cannot hold good in reſpect to the phlogiſton of phoſphorus, ſulphur, charcoal, &c. But all theſe ſubſtances contain other matters beſides phlogiſton and heat. The acids in the ſulphur and phoſphorus, and the alkali and earth in charcoal, may attract the phlogiſton ſo powerfully that the heat they contain may not be able to overcome the adheſion of their particles, until, by the effect of external heat, they are once removed to ſuch a diſtance from one another as to be out of the ſphere of that kind of attraction *.

If it be found to be a conſtant fact, that equal additions of phlogiſton to dephlogiſticated air do not extricate equal quantities of heat, that may afford the means of finding the quantities of heat contained in phlogiſton and dephlogiſticated air reſpectively, and ſolve the problem.

Many other ideas on theſe ſubjects preſent themſelves; but I am not bold enough to trouble you, or the public, with any ſpeculations, but ſuch as I think are ſupported by uncontroverted facts.

I muſt therefore bring this long letter to a concluſion, and leave to others the future proſecution of a ſubject which, however engaging, my neceſſary avocations prevent me from purſuing. I cannot however conclude, without acknowledging my obligations to Dr. PRIESTLEY, who has given me every information and aſſiſtance in his power, in the courſe of my enquiries, with that candour and liberality of ſentiment which diſtinguiſh his character.

I return you my thanks for the obliging attention you have paid to this hypotheſis; and remain, with much eſteem, &c.

JAMES WATT.

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* On the whole, this queſtion ſeems to involve ſo many difficulties, that it cannot be cleared up without many new experiments.

XXVI. *Sequel to the Thoughts on the conſtituent Parts of Water and Dephlogiſticated Air. In a ſubſequent Letter from Mr.* James Watt, *Engineer, to Mr.* De Luc, *F. R. S.*

Read May 6, 1784.

Birmingham,
April 30, 1784.

DEAR SIR,

ON re-conſidering the ſubject of my letter to you of the 26th of November laſt, I think it neceſſary to reſume the ſubject, in order to mention ſome neceſſary cautions to thoſe who may chuſe to repeat the experiments mentioned there, and to point out ſome circumſtances that may cauſe variations in the reſults.

In experiments where the dephlogiſticated air is to be diſtilled from common or cubic nitre, theſe ſalts ſhould be purified as perfectly as poſſible, both from other ſalts and from phlogiſtic matter of any kind; otherwiſe they will produce ſome nitrous air, or yellow fumes, which will leſſen the quantity, and, perhaps, debaſe the quality of the dephlogiſticated air. If the nitre is perfectly pure, no yellow fumes are perceptible, until the alkaline part begins to act upon the glaſs of the retort, and even then they are very ſlightly yellow.

When earthen retorts are uſed, and a large quantity of air is drawn from the nitre, it acts very much upon the retort, diſſolves a great part of it, and becomes very alkaline, retaining only a ſmall part of its acid, at leaſt only a ſmall part which can be made appear in any of the known forms of that acid; and unleſs retorts can be obtained of a true apyrous and compact porcelain, I ſhould prefer glaſs retorts, properly coated, for making experiments for the preſent purpoſe.

In ſome of my experiments the nitre was left in the retort placed in a furnace, ſo that it took an hour or more to cool. In theſe caſes there was always a deficience of the acid part, which ſeemed, from ſome appearances on the coating, either to have penetrated the hot and ſoft glaſs, by paſſing from particle to particle, or to have eſcaped by ſmall cracks which happened in the retort during the cooling. There was the leaſt deficience of the acid when the diſtillation was performed as quickly as was practicable, and the retort was removed from the fire immediately after the operation was finiſhed. In order to ſhorten the duration of the experiment, and conſequently to leſſen the action of the nitre on the retort, it is adviſable not to diſtil above 50 ounce meaſures of dephlogiſticated air from an ounce of nitre. The experiment has ſucceeded beſt when the retort was placed in a charcoal fire in a chafing-diſh or open furnace; becauſe it is eaſy in that caſe to ſtop the operation, and to withdraw the retort at the proper period.

When the dephlogiſticated air is diſtilled from the nitre of mercury, the ſolution ſhould be performed in the retort itſelf, and the nitrous air produced by the ſolution ſhould be caught in a proper receiver, and decompoſed by the gradual admiſſion of common air through water; and the water, which thus becomes impregnated with the acid of the nitrous air, ſhould be added after the proceſs to the water through which the dephlogiſticated air has paſſed. When the ſolution ceaſes to give any more nitrous air, the point of the tube of the retort ſhould be raiſed out of the water; otherwiſe, by the condenſation of the watery and acid vapours which follow, a partial exhauſtion will take place, and the receiving water will riſe up into the retort and break it, or at leaſt ſpoil the experiment. A common receiver, ſuch as is uſed in diſtilling ſpirit of nitre, ſhould be applied, with a little water in it, to receive the acid ſteam; and it ſhould be kept as cool as can conveniently be done, as theſe fumes are very volatile. This receiver ſhould remain as long as the fumes are colourleſs; but when they appear, in the neck of the retort, of a yellow colour, it is a mark that the mercurial nitre will immediately produce dephlogiſticated air; the receiver ſhould then be withdrawn, and an apparatus placed to receive the air. The reſt of the proceſs has been ſufficiently explained in my former letter.

The phlogiſticated nitrous acid, ſaturated by an alkali, will not cryſtallize; and, if expoſed to evaporation, even in the heat of the air, will become alkaline again, which ſhews the weakneſs of its affinity with alkalies when diſſolved in water *; a farther proof of which is, that it is expelled from them by all the acids, even by vinegar (which fact has been obſerved by Mr. SCHEELE). I have obſerved, that litmus is no teſt of the ſaturation of this acid by alkalies; for the infuſion of litmus added to ſuch a mixture will turn red, when the liquor appears to be highly alkaline, by its turning the infuſions of violets, roſe leaves, and moſt other red juices, green. This does not proceed from the infuſion of litmus being more ſenſible to the preſence of acids than other teſts; for I have lately diſcovered a teſt liquor (the preparation of which I mean to publiſh ſoon) which is more ſenſible to the preſence of acids

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* You have informed me, that Mr. CAVENDISH has alſo obſerved this fact; and that he has mentioned it in a paper lately read before the Royal Society; but I had obſerved the fact previous to my knowledge of his paper.

than litmus is; but which turns green in the ſame ſolution of phlogiſticated nitre that turns litmus red.

The unavoidable little accidents which have attended theſe experiments, and which tend to render their reſults dubious, have prevented me from relying on them as *full* proofs of the poſition that no acid enters into the compoſition of dephlogiſticated air; though they give great probability to the ſuppoſition. I have, therefore, explained the whole of the hypotheſis and experiments with the diffidence which ought to accompany every attempt to account for the phænomena of nature on other principles than thoſe which are commonly received by philoſophers in general. And in purſuance of the ſame motives it is proper to mention, that the alkali employed to ſaturate the phlogiſticated nitrous acid, was always that of tartar which is partly mild; and I have not examined whether highly phlogiſticated nitrous acid can perfectly expel fixed air from an alkali, though I know no fact which proves the contrary. It ſhould alſo be examined, whether the ſame quantity of real nitrous acid is requiſite to ſaturate a given quantity of alkali, when the acid is phlogiſticated, as is neceſſary when it is dephlogiſticated.

As I am informed that you have done me the honour to communicate my former letter on this ſubject to the Royal Society, I ſhall be obliged to you to do me the ſame favour in reſpect to the preſent letter, if you judge that it merits it.

I remain, &c.

JAMES WATT.

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XXVII. *An Attempt to compare and connect the Thermometer for ſtrong Fire, deſcribed in Vol. LXXII. of the Philoſophical Tranſactions, with the common Mercurial Ones. By Mr.* Joſiah Wedgwood, *F. R. S. Potter to Her Majeſty.*

Read May 13, 1784.

THIS thermometer, which I had the honour of laying before the Royal Society in May 1782, has now been found, from extenſive experience, both in my manufactories and experimental enquiries, to anſwer the expectations I had conceived of it as a meaſure of all degrees of common fire above ignition: but at preſent it ſtands in a detached ſtate, not connected with any other, as it does not begin to take place till the heat is too great to be meaſured or ſupported by mercurial ones.

What is now therefore wanting, to give us clear ideas of the value of its degrees, is, to connect it with one which long uſe has rendered familiar to us; ſo that if the ſcale of the common thermometer be continued indefinitely upwards as a ſtandard, the diviſions of mine may be reduced to that ſcale, and we may thus have the whole range of the degrees of heat brought into one uniform ſeries, expreſſed in one language, and comparable in every part, from the loweſt that have hitherto been produced by any artificial freezing mixtures, up to the higheſt that can be obtained in our furnaces, or that the materials of our furnaces and veſſels can ſupport.

The hope of attaining this deſirable and important object gave riſe to the experiments which I have now the honour of communicating. How far I may have ſucceeded, or whether the means employed were adequate to the end propoſed, is, with all deference, ſubmitted to this illuſtrious Society.

This attempt is founded upon the conſtruction and application of an intermediate meaſure, which takes in both the heats that are meaſurable by the mercurial thermometer, and a ſufficient number of thoſe that come within the province of mine to connect the two together; the manner of doing which will be apparent from the three firſt figures (tab. XIV.); wherein F repreſents FAHRENHEIT's thermometer, with a continuation of the ſcale; W my thermometer; and M the intermediate meaſure divided into any number of equal parts at pleaſure.

For if the heat of boiling water, or 212 degrees of FAHRENHEIT, be communicated to M, and its meaſure upon M marked, as at *a*; and if the heat of boiling mercury, or 600° of FAHRENHEIT, be alſo communicated to M, and marked as at *b*; it is plain, that the number of degrees upon M between *a* and *b* will be equal to the interval between 212 and 600, that is, to 388° upon FAHRENHEIT.

In like manner, upon expoſing M to two different heats above ignition along with my thermometer pieces, if a certain degree of my ſcale be found to correſpond with the point *d*, and another degree of mine with the point *c*; then the interval between thoſe two degrees upon mine muſt be equal to the interval *dc*; and how many of FAHRENHEIT's that interval is equivalent to will be known from the preceding compariſon. Thus we can find the number of FAHRENHEIT's degrees contained in any given extent of mine, and the degree of FAHRENHEIT's with which a given point of mine coincides;

whence either ſcale is eaſily reducible to the other through their whole range, whether we ſuppoſe Fahrenheit's continued upwards, or mine downwards.

For obtaining the intermediate thermometer different means were thought of; but the only principle which, upon attentive conſideration, afforded any proſpect of ſucceſs, was the *expanſion of metals.* This therefore was adopted, and among different methods of meaſuring that expanſion, which either occurred to myſelf, or which I can find to have been practiſed by others, there is no one which promiſes either ſo great accuracy, or convenience in uſe, as a gage like that by which the thermometer pieces are meaſured: the utility of this gage had now been confirmed to me by experience, and the machines and long rods, which have been employed for meaſuring expanſions on other occaſions, were abſolutely inadmiſſible here, on account of the inſuperable difficulties of performing nice operations of this kind in a red heat, and of communicating a perfectly equal heat through any conſiderable extent.

To give a clearer idea of this ſpecies of gage, which, ſimple as it is, I am informed has been miſunderſtood by ſome of the readers of my former paper, a repreſentation of one uſed on the preſent occaſion is annexed in fig. 4. where ABCD is a ſmooth flat plate; and EF and GH two rulers or flat pieces, a quarter of an inch thick, fixed flat upon the plate, with the ſides that are towards one another made perfectly true, a little further aſunder at one end EG than at the other end FH; thus they include between them a long converging canal, which is divided on one ſide into a number of ſmall equal parts, and which may be conſidered as performing the offices both of the tube and ſcale of the common thermometer. It is obvious, that if a body, ſo adjuſted as to fit exactly at the wider end of this canal, be afterwards diminiſhed in its bulk by fire, as the thermometer pieces are, it will then paſs further in the canal, and more and more ſo according as the diminution is greater; and converſely, that if a body, ſo adjuſted as to paſs on to the narrow end, be afterwards expanded by fire, as is the caſe with metals, and applied in that expanded ſtate to the ſcale, it will not paſs ſo far; and that the diviſions on the ſide will be the meaſures of the expanſions of the one, as of the contractions of the other, reckoning in both caſes from that point to which the body was adjuſted at firſt.

I is the body whoſe alteration of bulk is thus to be meaſured, which, in the preſent inſtance, is a piece of fine ſilver: this is to be gently puſhed or ſlid along, towards the end FH, till it is ſtopped by the converging ſides of the canal.

K is a little veſſel formed in the gage for this particular ſeries of experiments, the uſe of which will appear hereafter.

The *contraction*, which the thermometer pieces receive from fire, is a permanent effect, not variable by an abatement of the heat, and which accordingly is meaſured commodiouſly and at leiſure, when the pieces are grown cold. But the *expanſion* of bodies is only temporary, continuing no longer than the heat does that produced it; and therefore its quantity, at any particular degree of heat, muſt be meaſured in the moment while that heat ſubſiſts. And further, if the heated piece was applied to the cold gage, the piece would be deprived of a part of its heat on the firſt contact; and as the gage receives ſome degree of expanſion from heat as well as the piece, it is plain that in this caſe the piece would be diminiſhed in its bulk, and the gage enlarged, before the meaſurement could be taken. It is therefore neceſſary that both of them be heated to an exact equality; and in that ſtate we can meaſure, not indeed the *true expanſion of* *either*, but the *exceſs* of the expanſion of one above that of the other, which is ſufficient for the preſent purpoſe, as we want only an uniform and graduated effect of fire, and it is totally immaterial whether that effect be the abſolute expanſion of one or the other body, or the difference of the two, provided only that its *quantity* be ſufficient to admit of nice meaſurement.

Some difficulties occurred with reſpect to the choice of a proper *matter* for the gage; the eſſential requiſites of which are, to have but little expanſibility, and to bear the neceſſary fires without injury. All the metals, except gold and ſilver, would calcine in the fire: thoſe two are indeed free from that objection, and accordingly it is of the moſt expanſible of them that the piece is made; but if the gage alſo was made of the ſame, the meaſure itſelf would expand juſt as much as the body to be meaſured, and no expanſion at all would be ſenſible; and though the gage was made of one of thoſe metals, and the piece of the other, the difference between their expanſions would be too ſmall to give any ſatisfactory reſults, as more than two-thirds of the real expanſion of either would be loſt or taken off by the other.

For theſe reaſons I had recourſe to earthy compoſitions, which expand by heat much leſs than metallic bodies, and bear the neceſſary degrees of fire without the leaſt injury. I made choice of tobacco-pipe clay, mixed with charcoal in fine powder, in the proportion of three parts of the charcoal to five of the clay by weight. By a free acceſs of air, in the burning by which the gage is prepared for uſe, the charcoal is conſumed, and leaves the clay extremely light and porous; from which circumſtance it bears ſudden alternations of cold and heat, often requiſite in theſe operations, much better than the clay alone. Another and more important motive for the uſe of charcoal was, that in conſequence of the remarkable poroſity which it produces in the clay, it would probably diminiſh the expanſibility, by occaſioning the maſs to contain, under an equal ſurface, a much leſs quantity of ſolid or expanſible matter. It may be objected to this idea, that the expanſions of metals, in Mr. ELLICOTT's * and Mr. SMEATON's † experiments, do not appear to have any connection at all with their denſities: but the caſes are by no means parallel; for there the compariſon lies between different ſpecies of matter; but here, between one and the ſame matter in different ſtates of compactneſs. If a metal could be treated as clay is in this inſtance, that is, if a large bulk of any foreign matter could be blended with it, and this matter afterwards burnt out, ſo as to leave the metallic particles at the ſame diſtances to which they had been ſeparated by the mixture of it, we may preſume that the metal thus enlarged would not expand ſo much as an equal volume of the ſolid metal. Such at leaſt were the ideas which determined my choice to a compoſition of clay and charcoal powder; and being afterwards deſirous of ſatisfying myſelf whether they had any foundation in fact, I have, ſince the experiments were made, prepared ſome pieces of clay with and without charcoal, and having burnt them in the ſame fire, I ground them at the ſides, to make them both fit exactly to the ſame diviſion near the narrow end of the gage; then, examining their expanſions by equal heats, I found the piece with charcoal to expand only one-third part ſo much as that without; and thus was fully ſatisfied with the compoſition of the gage.

To aſcertain a fixed point on the ſcale for the diviſions to be counted from, the ſilver piece and gage were laid together for

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* Phil. Tranſact. vol. XLVII. p. 485.

† Ibid. vol. XLVIII. p. 612.

ſome time in ſpring water, of the temperature of 50° of FAHRENHEIT: the point which the piece went to in this cold ſtate is that marked 0 near the narrow end of the gage. The adjuſtment is re-examined at the beginning and end of every ſucceeding experiment, leſt the repeated attrition, in ſliding the piece backwards and forwards, ſhould wear off ſo much from the ſurface of this ſoft metal as to occaſion an error in the minute quantities here meaſured.

The apparatus is then expoſed ſucceſſively to different degrees of heat, with the piece lying always in a part of the canal at leaſt as wide as it is expected to fill when expanded, otherwiſe the ſides of the gage would be burſt aſunder by its expanſion, as I experienced in ſome of my firſt trials. When the whole has received any particular degree of heat deſired, the piece is cautiouſly and equably puſhed along, till it is ſtopped by the convergency of the ſides, of which I always find notice given me by the gage itſelf (which is ſmall and light) beginning to move upon the continuance of the impulſe. A flat ſlip of iron, a little narrower than the piece, bent down to a right-angle at one end, and fixed in a long handle at the other, makes a convenient inſtrument for puſhing the piece forward, or drawing it back again, whilſt red-hot: this inſtrument, at every time of uſing, is heated to the ſame degree as the piece itſelf.

The heat of boiling water is taken without difficulty, by keeping the apparatus in boiling water itſelf during a ſufficient ſpace of time for the full heat to be communicated to it. The water I made uſe of was a very fine ſpring water, which on chemical trials appeared very nearly equal in purity to that of rain or ſnow; and I had previouſly ſatisfied myſelf, by trials in the cold, that the gage and piece being wet, or under water, made no difference in the meaſurement. The expanſion of the ſilver by this heat, that is, by an increaſe of the heat from 50° to 212°, or a period containing 162° of FAHRENHEIT, was juſt 8° of the gage or intermediate thermometer M; whence one of theſe degrees, according to this experiment, contains juſt 20°¼ of FAHRENHEIT'S. The operation was many times repeated, and the reſult was always preciſely the ſame.

For the boiling heat of mercury, it was neceſſary to proceed in a different manner; not to convey the heat from the mercury to the inſtrument, but to convey it equally to them both from another body. I made a ſmall veſſel for holding the mercury in the gage itſelf, ſeen at K fig. 4. and more diſtinctly in fig. 5. which is a tranſverſe ſection of the gage through this veſſel. The plate CD, which forms the bottom of the canal, ſerves alſo for the bottom of the veſſel, which is ſituated cloſe to the ſide of the canal, and as near as could be to that part of it, in which both the ſilver piece, and the diviſions required for this particular experiment, are contained. By this arrangement it is preſumed, that all the parts concerned in the operation will receive very nearly an equal heat.

The gage, with ſome mercury in the veſſel, was laid upon a ſmooth and level bed of ſand, on the bottom of an iron muffle kept open at one end; the fire increaſed very gradually till the mercury boiled, and then continued ſteady, ſo as juſt to keep it boiling, for a conſiderable time. The boiling heat of mercury was thus found to be 27°½ of the intermediate thermometer, which anſwering to an interval of 550° of FAHRENHEIT, makes one degree of this equal to juſt 20° of his; a reſult correſponding even beyond my expectations with that which boiling water had given.

Theſe ſtandard heats of FAHRENHEIT'S thermometer are obtained with little difficulty on a common fire; but it is far otherwiſe with the higher ones in which mine begins to apply; and all the precautions I could take, by uſing a cloſe muffle, ſurrounding it as equally as poſſible with the fuel, varying its poſition with reſpect to the draught of air, &c. proved inſufficient for ſecuring the neceſſary equality of heat even through the ſmall ſpace concerned in theſe experiments. Nor had I any idea, before the diſcovery of this thermometer, of the extreme difficulty, not to ſay impracticability, of obtaining, in common fires, or in common furnaces, an uniform heat through the extent even of a few inches. Incredible as this may appear at firſt ſight, whoever will follow me in the operations I have gone through, placing accurate meaſures of the heat in different parts of one and the ſame veſſel, will ſoon be convinced of its truth, and that he can no otherwiſe expect to communicate with certainty an equal heat to different pieces, than by uſing a fire of ſuch magnitude as to exceed perhaps ſome hundreds of times the bulk of the matters required to be heated.

To ſuch large body of fire, therefore, after many fruitleſs attempts in ſmall furnaces, not a little diſcouraging by the irregularity of their reſults, I at length had recourſe, fitting up for this purpoſe an iron oven, uſed for the burning-on of enamel colours upon earthen ware, about four feet long, by two and a half wide, and three feet high, which is heated by the flame of wood conducted all round it. An iron muffle, four inches wide, two inches and three quarters high, and ten inches long, containing the gage and piece, was placed in the middle of this oven, and the vacancy between them filled up with earthen ware, to increaſe the quantity of ignited matter, and thereby communicate the heat more equably from the oven to the muffle. In ſuch a ſituation of the muffle, in the center of an oven more than five hundred times its own capacity, it could not well fail of being heated pretty uniformly, at leaſt through the ſmall ſpace which theſe experiments required; nor have I found any reaſon to ſuſpect that it was not ſo.

The gage being laid flat upon the bottom of the muffle, with the ſilver piece in the canal as before, ſome of the clay thermometer pieces were ſet on end upon the ſilver piece, with that end of each downwards which is marked to go foremoſt in meaſuring it; that is, they were in contact with the ſilver in that part of their ſurface by which their meaſure is afterwards aſcertained. I was led to this precaution by an experiment I had made upon another occaſion, in which a number of thermometer pieces having been ſet upright upon an earthen-ware plate, over a ſmall fire, till the plate became red-hot, all the pieces were found diminiſhed, ſome of them more than two degrees, at the lower ends which reſted upon the plate, whilſt the upper ends were as much enlarged, not having yet paſſed the ſtage of extenſion which, as obſerved in the former paper, always precedes the thermometric diminution: thus we ſee how punctually every part of the piece obeys the heat that acts upon it.

The fire about the oven was ſlowly increaſed for ſome hours, and kept as even and ſteady as poſſible, by an experienced fireman, under my own inſpection. Upon opening a ſmall door, which had been made for introducing the apparatus, and looking in from time to time, it was obſerved, that the muffle, with the adjacent parts of the oven and ware, acquired a viſible redneſs at the ſame time; and in the progreſs of the operation, the eye could not diſtinguiſh the leaſt diſſimilarity in the aſpect of the different parts; whereas in ſmall fires, the difference not only between the two ends of the muffle, but in much leſs diſtances, is ſuch as to ſtrike the eye at once.

When the muffle appeared of a low red heat, ſuch as was judged to come fully within the province of my thermometer, it was drawn forward, towards the door of the oven; and its own door being then nimbly opened by an aſſiſtant, I immediately puſhed the ſilver piece as far as it would go. But as the diviſion which it went to could not be diſtinguiſhed in that ignited ſtate, the muffle was lifted out, by means of an iron rod paſſed through two rings made for that purpoſe, with care to keep it ſteady, and avoid any ſhake that might endanger the diſplacing of the ſilver piece.

When grown ſufficiently cold to be examined, I noted the degree of expanſion which the ſilver piece ſtood at, and the degree of heat ſhewn by the thermometer pieces meaſured in their own gage; then returned the whole into the oven as before, and repeated the operation with a ſtronger heat, to obtain another point of correſpondence on the two ſcales.

The firſt was at 2°¼ of my thermometer, which coincided with 66° of the intermediate one; and as each of theſe laſt has been before found to contain 20 of Fahrenheit's, the 66 will contain 1320; to which add 50, the degree of his ſcale to which the 0 of the intermediate thermometer was adjuſted, and the ſum, 1370, will be the degree of Fahrenheit's correſponding to my 2°¼.

The ſecond point of coincidence was at 6°¼ of mine, and 92° of the intermediate; which 92 being, according to the above proportion, equivalent to 1840 of Fahrenheit, add 50 as before to this number, and my 6°¼ is found to fall upon the 1890th degree of Fahrenheit.

It appears from hence, that an interval of 4 degrees upon mine is equivalent to an interval of 520° upon his; conſequently 1 of mine to 130 degrees of his; and that the 0 of mine correſponds to his 1077°½. Several other trials were made, which gave reſults ſo nearly alike, that I have little apprehenſion of any material error.

From theſe data it is eaſy to reduce either ſcale to the other through their whole range; and from ſuch reduction it will appear, that an interval of near 480° remains between them, which the intermediate thermometer ſerves as a meaſure for; that mine includes an extent of about 32000 of FAHRENHEIT's degrees, or about 54 times as much as that between the freezing and boiling points of mercury, by which mercurial ones are naturally limited; that if the ſcale of mine be produced downwards, in the ſame manner as we have ſuppoſed FAHRENHEIT's to be produced upwards, for an ideal ſtandard, the freezing point of water would fall nearly on 8° below 0 of mine, and the freezing point of mercury a little below 8°½; and that, therefore, of the extent of now meaſurable heat, there are about $\frac{5}{10}$ths of a degree of my ſcale from the freezing of mercury to the freezing of water; 8° from the freezing of water to full ignition; and 160° above this to the higheſt degree I have hitherto attained.

As we are now enabled to compare not only the higher degrees among themſelves, and the lower among themſelves, upon their reſpective ſcales, but likewiſe the higher and lower with each other in every ſtage, it may be proper to take a general view of the whole range of meaſurable heat, as expreſſed both in FAHRENHEIT's denominations and in mine; and for this purpoſe I have drawn up a little table of a few of the principal points that have been aſcertained, to ſhew their mutual relations or proportions to each other: any other points that have been, or hereafter may be, obſerved, by theſe or any other known thermometers, may be inſerted at pleaſure.

	Fahr.	Wedg.
Extremity of the ſcale of my thermometer	32277°	240°
Greateſt heat of my ſmall air-furnace -	21877	160
Caſt iron melts - - - -	17977	130
Greateſt heat of a common ſmith's forge	17327	125
Welding heat of iron, greateſt -	13427	95
——————— leaſt - -	12777	90
Fine gold melts - - -	5237	32
Fine ſilver melts - - -	4717	28
Swediſh copper melts - -	4587	27
Braſs melts - - -	3807	21
Heat by which my enamel colours are burnt on	1857	6
Red-heat fully viſible in day-light -	1077	0
Red-heat fully viſible in the dark -	947	1
Mercury boils - - -	600	$3\frac{673}{1000}$
Water boils - - -	212	$6\frac{658}{1000}$
Vital heat - - -	97	$7\frac{542}{1000}$
Water freezes - - -	32	$8\frac{42}{1000}$
Proof ſpirit freezes - - -	0	$8\frac{289}{1000}$
The point at which mercury congeals, conſequently the limit of mercurial thermometers, - -	about 40	$8\frac{596}{1000}$

To aſſiſt our conceptions of this ſubject, it may be proper to view it in another light, and endeavour to preſent it to the eye; for *numbers*, on a high ſcale, are with difficulty eſtimated and compared by the mind. I have therefore completed the ſcales of which a part is repreſented in fig. 1. and 3. by continuing the ſame equal diviſions, both upwards and downwards, as far as the utmoſt limits of heat that have hitherto been attained and meaſured*.

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* Mr. Wedgwood preſented this, in the form of a very long roll, to the Society.

In a ſcale of heat drawn up in this manner, the comparative extents of the different departments of this grand and univerſal agent are rendered conſpicuous at a ſingle glance of the eye. We ſee at once, for inſtance, how ſmall a portion of it is concerned in animal and vegetable life, and in the ordinary operations of nature. From freezing to vital heat is barely a five-hundredth part of the ſcale; a quantity ſo inconſiderable, relatively to the whole, that in the higher ſtages of ignition, ten times as much might be added or taken away, without the leaſt difference being diſcernible in any of the appearances from which the intenſity of fire has hitherto been judged of. From hence, at the ſame time, we may be convinced of the utility and importance of a phyſical meaſure for theſe higher degrees of heat, and the utter inſufficiency of the common means of diſcriminating and eſtimating their force. I have too often found differences, aſtoniſhing when conſidered as a part of this ſcale, in the heats of my own kilns and ovens, without being perceivable by the workmen at the time, or till the ware was taken out of the kiln.

SINCE the foregoing experiments were made, I have ſeen a very curious Memoir by Meſſ. LAVOISIER and DE LA PLACE, containing a method of meaſuring heat by the quantity of ice which the heated body is capable of liquefying. The application of this important diſcovery, as an intermediate ſtandard meaſure between FAHRENHEIT's thermometer and mine, could not eſcape me, and I immediately ſet about preparing an apparatus, and making the experiments neceſſary for that purpoſe; in hopes either of attaining by this method a greater degree of accuracy than I could expect from any other means, or of having what I had already done confirmed by a ſeries of experiments upon a different principle.

But in the proſecution of theſe experiments I have, to my great mortification, hitherto failed of ſucceſs; and I ſhould have contented myſelf for the preſent with ſaying little more than this, if ſome phænomena had not occurred, which appear to me not unworthy of farther inveſtigation.

The authors obſerve, that if ice, cooled to whatever degree below the freezing point, be expoſed to a warmer atmoſphere, it will be brought up to the freezing point through its whole maſs before any part of its ſurface begins to liquefy; and that conſequently ice, beginning to melt on the ſurface, will be always exactly of the ſame temperature, *viz.* at the freezing point; and that if a heated body be incloſed in a hollow ſphere of ſuch ice, the whole of its heat will be taken up in liquefying the ice; ſo that if the ice be defended from external warmth, by ſurrounding it with other ice in a ſeparate veſſel, the weight of the water produced from it will be exactly proportional to the heat which the heated body has loſt; or, in other words, will be a true phyſical meaſure of the heat.

For applying theſe principles in practice, they employ a tin veſſel, divided, by upright concentric partitions, into three compartments, one within another. The innermoſt compartment is a wire cage, for receiving the heated body. The ſecond, ſurrounding this cage, is filled with pounded ice, to be melted by the heat; and the outermoſt is filled alſo with pounded ice, to defend the former from the warmth of the atmoſphere. The firſt of theſe ice compartments terminates at bottom in a ſtem like a funnel, through which the water is conveyed off; and the other ice compartment terminates in a ſeparate canal, for diſcharging the water into which *that* ice is reduced. As ſoon as the heated body is dropped into the cage, a cover is put on, which goes over both that and the firſt ice compartment; which cover is itſelf a kind of ſhallow veſſel, filled with pounded ice, with holes in the bottom for permitting the water from this ice to paſs into the ſecond compartment, all the liquefaction that happens here, as well as there, being the effect of the heated body only. Over the whole is placed another cover with pounded ice, as a defence from external warmth.

As ſoon as this diſcovery came to my knowledge, on the 23d of February, a thaw having begun three days before, after a froſt which had continued with very little intermiſſion from the 24th of December, I collected a quantity of ice, and ſtored it up in a large caſk in a cellar.

I thought it neceſſary to ſatisfy myſelf in the firſt place, by actual experiment, that ice, how cold ſoever it may be, comes up to the freezing point through its whole maſs before it begins to liquefy on the ſurface. For this purpoſe I cooled a large fragment of ice, by a freezing mixture, to 17° of FAHRENHEIT's thermometer, and then hung it up in a room whoſe temperature was 50°. When it began to drop, it was broken, and ſome of the internal part nimbly pounded and applied to the bulb of a thermometer that was cooled by a freezing mixture below 30°. The thermometer roſe to, and continued at, 32°; being then taken out, and raiſed by warmth to 40°, ſome more of the ſame ice, applied as before to the bulb, ſunk it again to 32°; ſo that no doubt could remain on this ſubject.

Apprehenſive that pounded ice, directed by the authors, might imbibe and retain more or leſs of the water by capillary attraction, according to circumſtances, and thereby occaſion ſome error in the reſults, I thought it neceſſary to ſatisfy myſelf in this reſpect alſo by experiment. I therefore pounded ſome ice, and laid it in a conical heap on a plate; and having at hand ſome water, coloured with cochineal, I poured it gently into the plate, at ſome diſtance from the heap: as ſoon as it came in contact with the ice, it roſe haſtily up to the top; and on lifting up the lump, I found that it held the water, ſo taken up, as a ſponge does, and did not drop any part of it till the heat of my hand, as, I ſuppoſe, began to liquefy the maſs. On further trials I found, that in pounded ice preſſed into a conical heap, the coloured water roſe, in the ſpace of three minutes, to the height of two inches and a half; and by weighing the water employed, and what remained upon the plate unabſorbed, it appeared, that four ounces of ice had thus taken up, and retained, one ounce of water.

To further aſcertain this abſorbing power, in different circumſtances, more analogous to thoſe of the proceſs itſelf, I preſſed ſix ounces of pounded ice pretty hard into the funnel, having firſt introduced a wooden core in order to leave a proper cavity in the middle: then, taking out the core, and pouring an ounce of water upon the ice, I left the whole for half an hour; at the end of which time the quantity that ran off was only 12 pennyweights and 4 grains, ſo that the ice had retained 7 pennyweights and 20 grains, which is nearly one-twelfth of its own weight, and two-fifths of the weight of the water.

Theſe previous trials determined me, inſtead of uſing pounded ice, to fill a proper veſſel with a ſolid maſs of ice, by means of a freezing mixture, as the froſt was now gone, and then expoſe it to the atmoſphere till the ſurface began to liquefy. The apparatus I fitted up for this purpoſe was made of earthen ware well glazed, and is repreſented in fig. 6. (tab. XV.).

A, is a large funnel, filled with a ſolid maſs of ice. B, a cavity in the middle of this ice, formed, part of the way, by ſcraping with a knife, and for the remaining part, by boring with a hot iron wire. C, one of my thermometer pieces, which ſerves for the heated body, and reſts upon a coil of braſs wire: it had previouſly been burnt with ſtrong fire, that there might be no danger of its ſuffering any further diminution of its bulk by being heated again for theſe experiments. D, a cork ſtopper in the orifice of the funnel. E, the exterior veſſel, having the ſpace between its ſides and the included funnel A, filled with pounded ice, as a defence to the ice in the funnel. F, a cover for this exterior veſſel, filled with pounded ice for the ſame purpoſe. G, a cover for the funnel, filled alſo with pounded ice, with perforations in the bottom for allowing the water from this ice to paſs down into the funnel.

The thermometer piece was heated in boiling water, taken up with a pair of ſmall tongs equally heated, dropped inſtantly into the cavity B, and the covers put on as expeditiouſly as poſſible; the bottom of the funnel being previouſly corked, that the water might be detained till it ſhould part with all its heat, and likewiſe to prevent the water from the other ice, which ran down on the outſide of the funnel, from mingling with it.

After ſtanding about ten minutes, the funnel was taken out, wiped dry, and uncorked over a weighed cup: the water that ran out weighed 22 grains. Thinking this quantity too ſmall, as the piece weighed 72 grains, I repeated the experiment, and kept the piece longer in the funnel; but the water this time weighed only 12 grains. Being much diſſatisfied with this reſult, I made a third trial, continuing the piece much longer in the cavity; but the quantity of water was now ſtill leſs, not amounting to quite three drops; and, to my great ſurpriſe, I found the piece frozen to the ice, ſo as not to be eaſily got off, though all the ice employed was, at the beginning of the experiment, in a thawing ſtate.

I had prepared the apparatus for taking the boiling heat of mercury; but being entirely diſcouraged by theſe very unequal reſults, I gave that up, for the preſent at leaſt, and heating the piece to 6° of my thermometer, turned it nimbly out of the caſe in which it was heated into the cavity, throwing ſome fragments of ice over it. In about half an hour, I drew off the water, which amounted to 11 pennyweights; then ſtopping the funnel again, and replacing the covers, I left the whole about ſeven hours.

At the end of that time, I found a conſiderable quantity of water in the funnel: the melting of the ice had produced a cavity between it and the ſides, great part of the way down, which, as well as that in the middle, was nearly full. The water nevertheleſs ran out ſo ſlowly, that I apprehended ſomething had ſtopped the narrow end of the funnel, but the true cauſe became afterwards apparent upon examining the ſtate of the ice. The fragments which I had thrown over the thermometer piece were frozen entirely together, and in ſuch a form as they could not have aſſumed without freſh water ſuperadded and frozen upon them, for the cavities between them were partly filled with new ice. I endeavoured to take the ice out with my fingers, but in vain; and it was with ſome difficulty I could force it aſunder even with a pointed knife, to get at the thermometer piece. When that was got out, great part of the coiled wire was found enveloped in new ice. The paſſage through the ice to the ſtem of the funnel, which I had made pretty wide with a thick iron wire red-hot, was ſo nearly cloſed up, that the ſlow draining off of the water was now ſufficiently accounted for, and indeed this draining was the only apparent mark of any paſſage at all. On taking the ice out of the funnel, and breaking it to examine this canal, I found it almoſt entirely filled up with ice projecting from the ſolid maſs in cryſtalline forms, ſimilar in appearance to the cryſtals we often meet with in the cavities of flints and quarzoſe ſtones.

If, after all theſe circumſtances, any doubt could have remained of the ice in queſtion being a new production, a fact which I now obſerved muſt have removed all ſuſpicion. I found a coating of ice, of conſiderable extent and perfectly tranſparent, about a tenth of an inch in thickneſs, upon the outſide of the funnel, and on a part of it which was not in contact with the ſurrounding ice, for that was melted to the diſtance of an inch from it.

Some of the ice being ſcraped off from the inſide of the funnel, and applied to the bulb of the thermometer, the mercury ſunk from 50° to 32°, and continued at that point till the ice was melted; after which, the water being poured off, it roſe in a little time to 47°.

Aſtoniſhed at theſe appearances, of the water freezing after it had been melted, though ſurrounded with ice in a melting ſtate, and in an atmoſphere about 50°, where no part of the apparatus or materials could be ſuppoſed to be lower than the freezing point, I ſuſpected at firſt that ſome of the ſalt of the freezing mixture might have got into the water, and that this, in diſſolving, might perhaps abſorb, from the parts contiguous to it, a greater proportion of heat than the ice of pure water does. But the water betrayed nothing ſaline to the taſte, and I had applied the freezing mixture with my own hands with great care, to prevent any of it being mixed with the water.

To remove all doubts, however, upon this point, I purpoſed repeating the experiment with ſome pieces of the ice I had ſtored up in the cellar, to ſee if this would congeal, after thawing, in the ſame manner. But going to fetch the ice, and examining it in the caſk in which it was kept, I was perfectly ſatisfied with the appearances I found there; for though much of it was melted, yet the fragments were frozen together, ſo that it was with difficulty I could break or get out any pieces of it with an iron-ſpade; and, when ſo broken, it had the appearance of *breccia* marble or plum-pudding ſtone, for the fragments had been broken and rammed into the caſk with an iron mall.

A porcelain cup being laid upon ſome of this ice about half an hour, in a room whoſe temperature was 50°, it was found pretty firmly adhering, and when pulled off, the ice exhibited an exact impreſſion of the fluted part of the cup which it had been in contact with; ſo that the ice muſt neceſſarily have liquefied firſt, and afterwards congealed again. This was repeated ſeveral times, with the ſame event. Fragments of the ice were likewiſe applied to one another, to ſponges, to pieces of flannel and of linen cloth, both moiſt and dry: all theſe, in a few ſeconds, began to cohere, and in about a minute were frozen ſo as to require ſome force to ſeparate them. After ſtanding an hour, the coheſion was ſo firm, that on pulling away the fragments of ice from the woollen and ſponge, they tore off with them that part of the ſurface which they were in contact with, though at the ſame time both the ſponge and flannel were filled with water which that very ice had produced.

To make ſome eſtimate of the force of the congelation, which was ſtronger on the two bodies laſt mentioned than on linen, I applied a piece of ice to a piece of dry flannel which weighed two pennyweights and a half, and ſurrounded them with other ice. After lying together three quarters of an hour, taking the piece of ice in my hand and hooking the flannel to a ſcale, I found a weight of five ounces to be neceſſary for pulling it off, and yet ſo much of the ice had liquefied as to increaſe the weight of the flannel above 12 pennyweights. I then weighed the piece of ice, put them together again, and four hours after found them frozen ſo firmly as to require 78 ounces for their ſeparation, although, from 42 pennyweights of the ice, 15 more had melted off: the ſurface of contact was at this time nearly a ſquare inch. I continued them again together for ſeven hours; but they now bore only 62 ounces, the ice being diminiſhed to 14 pennyweights, and the ſurface of contact reduced to about ſix-tenths of a ſquare inch.

Having ſeen before that pounded ice abſorbs water in very conſiderable quantity, I ſuſpected that ſomething of the ſame kind might take place even with entire maſſes; and experiment ſoon convinced me, that even apparently ſolid pieces of ice will imbibe water, ſlower or quicker according to its ſtage of decay. I have repeatedly heated ſome of my thermometer pieces, and laid them upon ice, in which they made cavities of conſiderable depth, but the water was always abſorbed, ſometimes as faſt as it was produced, leaving both the piece and the cavity dry.

Thus, though I cannot ſufficiently expreſs how much I admire the diſcovery that gave riſe to theſe experiments, I have nevertheleſs to lament my not being able to avail myſelf of it at preſent for the purpoſe I wiſhed to apply it to.

That in my experiments the two ſeemingly oppoſite proceſſes of nature, congelation and liquefaction, went on together, at

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the ſame inſtant, in the ſame veſſel, and even in the ſame fragment of ice, is a fact of which I have the fulleſt evidence that my ſenſes can give me; and I ſhall take the liberty of ſuggeſting a few hints, which may tend perhaps to elucidate their cauſe, and to ſhew that they are not ſo incompatible as at firſt ſight they appear to be.

It occurred to me at firſt, that water highly attenuated and divided, as when reduced into vapour, may freeze with a leſs degree of cold than water in its aggregate or groſſer form; hence hoar-froſt is obſerved upon graſs, trees, &c. at times when there is no appearance of ice upon water, and when the thermometer is above the freezing point*. BOERHAAVE, I find, in his elaborate theory of fire, aſſigns 33° as the freezing point of vapour, and even of water when divided only by being imbibed in a linen cloth.

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* I am aware, that experiments and obſervations of this kind are not fully deciſive; that the atmoſphere may, in certain circumſtances, be much warmer or colder than the earth and waters, which, in virtue of their denſity, are far more retentive of the temperature they have once received, and leſs ſuſceptible of tranſient impreſſions; that even inſenſible undulations of water, from the ſlighteſt motion of the air, by bringing up warmer ſurfaces from below, may prove a further impediment to the freezing; and, therefore, that the degree of cold, which is ſufficient to produce hoar-froſt, may poſſibly, if continued long enough, be ſufficient alſo to produce ice. I am not acquainted with any ſatisfactory experiments or obſervations yet made upon the ſubject; nor do I advance the principle as a certain, but as a probable one, which occurred to me at the moment, which is countenanced by general obſervation, and conſentaneous to many known facts; for there are numerous inſtances of bodies, in an extreme ſtate of diviſion, yielding eaſily to chemical agents which, before ſuch diviſion, they entirely reſiſt; thus ſome precipitates, in the very ſubtile ſtate in which they are at firſt extricated from their diſſolvents, are re-diſſolved by other menſtrua, which, after their concretion into ſenſible moleculæ, have no action upon them at all.

Now, as the atmoſphere abounds with watery vapour, or water diſſolved and chemically combined, and muſt be particularly loaded with it in the neighbourhood of melting ice; as the heated body introduced into the funnel muſt neceſſarily convert a portion of the ice or water there into vapour; and as ice is known to melt as ſoon as the heat begins to exceed 32°, or nearly one degree lower than the freezing point of vapour; I think we may from hence deduce, pretty ſatisfactorily, all the phænomena I have obſerved. For it naturally follows from theſe principles, that vapour may freeze where ice is melting; that the vapour may congeal even upon the ſurface of the melting ice itſelf; and that the heat which (agreeably to the ingenious theory of Dr. BLACK) it emits in freezing, may contribute to the further liquefaction of that very ice upon which the new congelation is formed.

I would further obſerve, that the freezing of water is attended with plentiful evaporation in a cloſe as well as an open veſſel, the vapour in the former condenſing into drops on the under ſide of the cover, which either continue in the form of water, or aſſume that of ice or a kind of ſnow, according to circumſtances*; which evaporation may perhaps be attributed to the heat that was combined with the water, at this moment rapidly making its eſcape, and carrying part of the aqueous fluid off with it. We are hence furniſhed with a freſh and continual ſource of vapour as well as of heat; ſo that the proceſſes of liquefaction and congelation may go on uninterruptedly together, and even neceſſarily accompany one another, although, as the freezing muſt be in an under proportion to the melting, the whole of the ice muſt ultimately be conſumed.

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* See Mr. BARON's paper on this ſubject, in the Memoires of the Academy of Sciences at Paris for the year 1753.

In the remarkable inſtance of the coating of ice on the outſide of the throat of the funnel, there are ſome other circumſtances which it may be proper to take notice of. Neither the cover of the outer veſſel, nor the aperture in its bottom which the ſtem of the funnel paſſed through, were air-tight, and the melting of the ſurrounding ice had left a vacancy of about an inch round that part of the funnel on which the cruſt had formed. As there was, therefore, a paſſage for air through the veſſel, a circulation of it would probably take place: the cold and denſe air in the veſſel would deſcend into the rarer air of the room then about 50°, and be replaced by air from above. The effect of this circulation and ſudden refrigeration of the air will be a condenſation of part of the moiſture it contains upon the bodies it is in contact with: the throat of the funnel, being one of thoſe bodies, muſt receive its ſhare; and the degree of cold in which the ice thaws being ſuppoſed ſufficient for the freezing of this moiſt vapour, the contact, condenſation, and freezing, may happen at the ſame inſtant.

The ſame principles apply to every inſtance of congelation that took place in theſe experiments; and a recollection of particulars which paſſed under my own eye convinces me, that the congelation was ſtrongeſt in thoſe circumſtances where vapour was moſt abundant, and on thoſe bodies which, from their natural or mechanic ſtructure, were capacious of the greateſt quantity of it; ſtronger, for inſtance, on ſponge than on woollen, ſtronger on this than on the cloſer texture of linen, and far ſtronger on all theſe than on the compact ſurface of porcelain.

If, nevertheleſs, the principle I have aſſumed (that water highly attenuated will congeal with a leſs degree of cold than water in the maſs) ſhould not be admitted; another has above been hinted at, which experiments have decidedly eſtabliſhed, from which the phænomena may perhaps be equally accounted for, and which, even though the other alſo is received, muſt be ſuppoſed to concur for ſome part of the effect; I mean, that *evaporation produces cold*; both vapour and ſteam carrying off ſome proportion of heat from the body which produces them. If, therefore, evaporation be made to take place upon the ſurface of ice, the contiguous ice will thereby be rendered colder; and as it is already at the freezing point, the ſmalleſt increaſe of cold will be ſufficient for freſh congelation. It ſeems to be on this principle that the formation of ice is effected in the Eaſt Indies, by expoſing water to a ſerene air, at the coldeſt ſeaſon of the year, in ſhallow porous earthen veſſels: part of the water tranſudes through the veſſel, and evaporating from the outſide, the remainder in the veſſel becomes cold enough to freeze; the warmth of the earth being at the ſame time intercepted by the veſſels being placed upon bodies little diſpoſed to conduct heat *. If ice is thus producible in a climate where natural ice is never ſeen, we need not wonder that congelation ſhould take place where the ſame principle operates amidſt actual ice.

It has been obſerved above, that the heat emitted by the congealing vapour probably unites with and liquefies contiguous portions of ice; but whether the whole, either of the heat ſo emitted, or of that originally introduced into the funnel, is thus taken up; how often it may unite with other portions of ice, and be driven out from other new congelations; whether there exiſts any difference in its chemical affinity or elective attraction to water in different ſtates and the contiguous bodies; whether part of it may not ultimately eſcape, without performing the office expected from it upon the ice; and to what diſtance from the evaporating ſurface the refrigerating effect of the evaporation may extend; muſt be left for further experiments to determine.

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* See a deſcription of this proceſs in the Philoſophical Tranſactions, vol. LXV. p. 253.

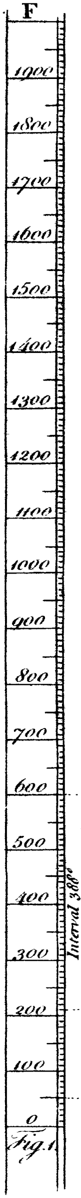


Fig. 1.

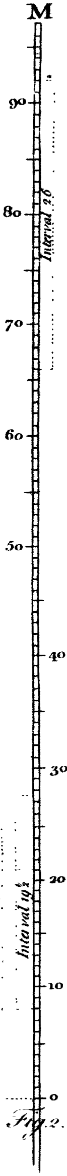


Fig. 2.

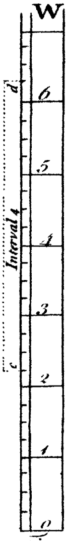


Fig. 3.

Fig. 4.

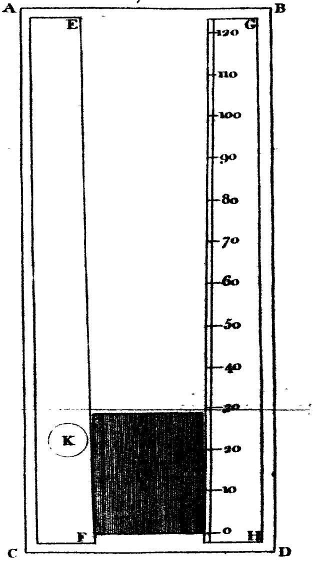


Fig. 5.

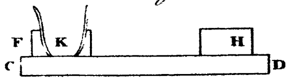
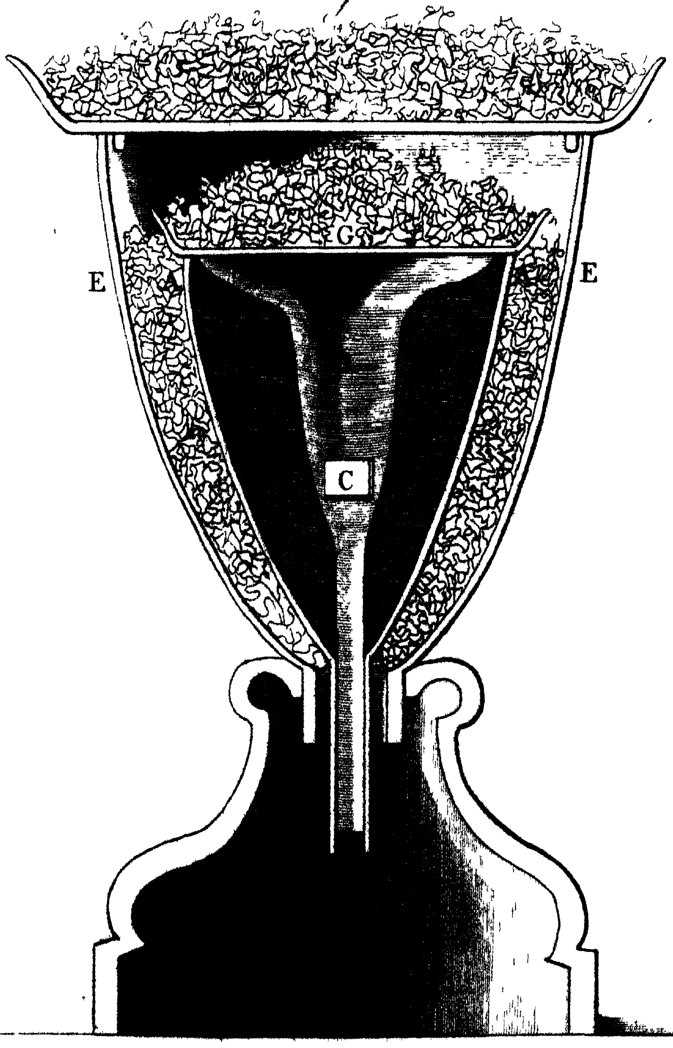


Fig. 6.



Basire s.

XXVIII. *On the Summation of Series, whoſe general Term is a determinate Function of z the Diſtance from the firſt Term of the Series. By* Edward Waring, *M. D.* Lucaſian *Profeſſor of the Mathematics at* Cambridge, *and Fellow of the Societies of* London *and* Bononia.

Read May 20, 1784.

PROBLEM.

THE ſum S *being given, to find a ſeries of which it is the ſum.*

1. Reduce the ſum S into a converging ſeries, proceeding according to the dimenſions of any ſmall quantities, and it is done. For example: let any algebraical function S of an unknown or ſmall quantity x be aſſumed, reduce it into a converging ſeries proceeding according to the dimenſions of x, and there reſults a ſeries whoſe ſum is S. 2. Let A, B, C, &c. be algebraical functions of x; reduce the $\int A\dot{x}$, $\int B\dot{x}$, $\int C\dot{x}$, &c. into a converging ſeries, proceeding according to the dimenſions of x, and the problem is done.

It is always neceſſary to find the values of x, between which the abovementioned ſerieſes converge. Reduce the algebraical function S in the firſt example, and the algebraical functions A, B, C, &c. in the ſecond into their loweſt terms; and in ſuch a manner, that the quantities contained in the numerator and denominator may have no denominator: make the denominator in the firſt example, and the denominator in the ſecond, and every diſtinct irrational quantity contained in them reſpectively $=0$; and alſo every diſtinct irrational quantity contained in the numerators $=0$. Suppoſe α the leaſt root affirmative or negative (but not $=0$) of the abovementioned reſulting equations; then a ſeries aſcending according to the dimenſions of x will always converge, if the value of x is contained between α and $-\alpha$; but if x be greater than α or $-\alpha$, the abovementioned ſeries will diverge. Let π be the greateſt root of the abovementioned reſulting equations; then a ſeries deſcending according to the reciprocal dimenſions of x will converge, if x be greater than $\pm\pi$; but, if leſs, not. When impoſſible roots $a \pm b\sqrt{-1}$ are contained in the equations, an aſcending ſeries will converge, if x be leſs than the leaſt root $\pm\alpha$, and $\pm(a-b)$ and $\pm(a+b)$; or more generally, if x be leſs than the leaſt root $\pm\alpha$, and x^{n+1} at an infinite diſtance n, be infinitely leſs than

$$\frac{2a^n - 2 \,.\, n \,.\, \frac{n-1}{2} a^{n-2} b^2 + 2 \,.\, n \,.\, \frac{n-1}{2} \,.\, \frac{n-2}{3} \,.\, \frac{n-3}{4} a^{n-4} b^4 - \&c.}{(a^2+b^2)^n};$$

a deſcending ſeries will always converge, when x is greater than the greateſt root of the reſulting equations; and x^{n-1}, when n is infinite, is infinitely greater than $(a+b)^n$ and $(a-b)^n$; or more generally than $2a^n - 2n \,.\, \frac{n-1}{2} a^{n-2} b^2 + 2n \,.\, \frac{n-1}{2} \,.\, \frac{n-2}{3} \,.\, \frac{n-3}{4} a^{n-4} b^4 - \&c.$

This follows from Caput 3. of the Meditationes Algebraicæ.

Cor. It appears from hence, that, if the aſcending ſeries converges, the deſcending will diverge; and, *vice verſâ*, if the deſcending converges, the aſcending will diverge, unleſs all the roots of the above-mentioned reſulting equations may be deemed of equal magnitude, as $+\alpha$ and α, $\alpha\sqrt[n]{-1}$, &c. and $x=\alpha$; in which caſe ſometimes both ſerieſes may become the ſame converging ſeries, &c.

When x, in the preceding caſes, is equal to the leaſt or greateſt root, the ſeries will ſometimes converge, and ſometimes not, as is ſhewn in the above-mentioned chapter. Whether the ſum of a ſeries, whoſe general term is given, can be found or not, will in many caſes appear from the law of the multinomial and other more general ſerieſes.

2. There are ſerieſes which always converge, whatever may be the value of x; as, for example, the ſeries $1+\frac{1}{2\cdot3}x+\frac{1}{2\cdot3\cdot4\cdot5}x^2+$&c. or $1+\frac{x}{2^2}+\frac{x^2}{3^3}+\frac{x^3}{4^4}+$&c. &c. always converge, whatever may be the value of x; but it may be obſerved, that theſe ſerieſes never ariſe from the expanſion of algebraical functions of x, or the before-mentioned fluents; but, in a few caſes, they may from fluxional equations. There are alſo ſerieſes which never converge as $1+1\cdot2x+1\cdot2\cdot3x^2+1\cdot2\cdot3\cdot4x^3+$&c. to which the preceding remark may be applied.

3. In the year 1757 ſome papers, which contained the firſt edition of my Meditationes Algebraicæ, were ſent to the Royal Society, in which was contained the following rule, *viz.* let S be a given function of the quantity x, which expand into a ſeries ($a+bx^m+cx^{2m}+$&c.) proceeding according to the dimenſions of x; in the quantity S, for x^m write αx^m, βx^m, γx^m, &c. where α, β, γ, &c. are roots of the equation $x^n-1=0$; and let the reſulting quantities be A, B, C, D, &c. then will $\frac{A+B+C+D+\&c.}{n}$ be equal to the ſum of the firſt, $2n+1$, $3n+1$, &c. terms *in infinitum.* This method, in the preface to the laſt

$\overline{x+e+\mu+2}^{\pi'} \times$ &c. $\times \overline{x+f}^{\rho} \times \overline{x+f+1}^{\rho} \times \overline{x+f+2}^{\rho} \ldots \times \overline{x+f+m-1}^{\rho} \times$ &c. $= D$, where π, π', ρ, &c.; μ, &c. are whole numbers; and the general term is $\frac{az^{b}+bz^{b-1}+cz^{b-2}+\&c.}{D}$ $= T''$; then, if the dimenſions of z in the numerator be leſs than its dimenſions in the denominator, will $T'' =$

$$\left(\frac{\alpha}{z+e}+\frac{\alpha'}{(z+e)^2}+\frac{\alpha''}{(z+e)^3}\ldots\frac{\alpha^{\pi+\pi'-1}}{(z+e)^{\pi+\pi'}}+\frac{\beta}{z+f}+\frac{\beta'}{(z+f)^2}+\frac{\beta''}{(z+f)^3}\ldots\frac{\beta^{\rho-1}}{(z+f)^{\rho}}+\&c.\right)+\left(\frac{\gamma}{z+e\,.\,z+e+1}+\frac{\theta}{z+f\,.\,z+f+1}+\&c.\right)+\&c.;$$

and in general there will be included all terms of the formulæ,

$$\frac{A\left(\overline{z+e+i}^{p}-\overline{z+e}^{p}\right)}{(z+e)^{p}\cdot(z+e+1)^{p}\ldots(z+e+i)^{p}},$$

$$\frac{B\left((z+f+i')^{p'}-(z+f)^{p'}\right)}{(z+f)^{p'}\cdot(z+f+1)^{p'}\ldots(z+f+i')^{p'}},\ \&c.$$

$$\frac{C\left((z+e+\mu+i'')^{p''}-(z+e+\mu)^{p''}\right)}{(z+e+\mu)^{p''}\cdot(z+e+\mu+1)^{p''}\ldots(z+e+\mu+i'')^{p''}},\ \&c.$$

where A, B, C, &c. α, α', &c. β, β', &c. γ, θ, &c. denote invariable quantities; and p, p', p'', &c. are whole numbers not greater than π, ρ, π', &c. reſpectively; and i, i', i'', &c. are whole numbers not greater than $n-1$, $m-1$, &c.

If all the quantities α, α', α'', &c. β, β', β'', &c. &c. are $= 0$, the ſum of the ſeries can be expreſſed in finite terms of the quantity z, otherwiſe not; and alſo if b be leſs than the dimenſions of z in the denominator by two or more, then will $\alpha+\beta+\&c. = 0$, otherwiſe the ſum would be infinite.

From $\pi+\pi'+\rho+\&c.-1$ independent ſums of infinite ſerieſes of this kind can be deduced the ſums of all infinite ſerieſes of the ſame kind.

This method may be extended to infinite ſeries, in which exponentials as e^z are contained, which will eaſily be ſeen from ſome ſubſequent propoſitions; but in my opinion the ſubſequent method of finding the ſum of ſerieſes is to be preferred to the preceding one, both for its generality and facility.

6. 1. Let the general term be $(az^b + bz^{b-1} + cz^{b-2} + \&c.) \times (z+e)^{-1} . (z+e+1)^{-1} . (z+e+2)^{-1} \ldots (z+e+n-1)^{-1}$; where b is a whole number leſs than n by two or more, when the ſum of an infinite ſeries is required.

Aſſume for the ſum the quantity $(z+e)^{-1} . (z+e+1)^{-1} . (z+e+2)^{-1} \ldots (z+e+n-2)^{-1} \times (\alpha z^{b'} + \beta z^{b'-1} + \gamma z^{b'-2} + \&c.)$; find the difference between this ſum and its ſucceſſive one $(z+e+1)^{-1} . (z+e+2)^{-1} . (z+e+3)^{-1} \ldots (z+e+n-1)^{-1} \times (\alpha \overline{z+1}^{b'} + \beta \overline{z+1}^{b'-1} + \&c.)$, which will be $-(z+e)^{-1} . (z+e+1)^{-1} . (z+e+2)^{-1} \ldots (z+e+n-1)^{-1} \times (\overline{z+e} \times (\alpha \overline{z+1}^{b'} + \beta \overline{z+1}^{b'-1} + \&c.) - \overline{z+e+n-1} \times (\alpha z^{b'} + \beta z^{b'-1} + \&c.) = \overline{b'-n+1} \alpha z^{b'} + \&c.)$; then make the terms of this difference equal to the correſpondent terms of the given quantity $az^b + bz^{b-1} + \&c.$ and there reſult $b' = b$, $-\overline{b-n+1} \times \alpha = a$, and conſequently $\alpha = \frac{-a}{b-n+1}$, &c.

2. Let the general term be $(z+e)^{-1} . (z+e+1)^{-1} . (z+e+2)^{-1} \ldots (z+e+n-1)^{-1} \times (z+f)^{-1} . (z+f+1)^{-1} . (z+f+2)^{-1} \ldots (z+f+m-1)^{-1} \times (az^b + bz^{b-1} + cz^{b-2} + \&c.)$. Aſſume the quantity $(z+e)^{-1} . (z+e+1)^{-1} \ldots (z+e+n-2)^{-1} \times (z+f)^{-1} . (z+f+1)^{-1} . (z+f+2)^{-1} \ldots (z+f+m-2)^{-1} \times (\alpha z^{b'} + \beta z^{b'-1} + \gamma z^{b'-2} + \&c.)$ for the ſum of the ſeries ſought; and thence deduce the general term, which ſuppoſe equal to the given general term, and from equating their correſponding parts eaſily can be deduced the index b' and co-efficients α, β, γ, &c. and conſequently the ſum of the ſeries ſought.

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3. Let the general term reduced to its lowest dimensions be $\overline{z+e}^{-\pi} \times \overline{z+e+1}^{-\pi} \dots \overline{z+e+n-1}^{-\pi} \times \overline{rz+f}^{-\rho} \times \overline{rz+f+r}^{-\rho} \times \overline{rz+f+2r}^{-\rho} \dots \overline{rz+f+\overline{m-1}r}^{-\rho} \times \overline{z+g}^{-\sigma} \times \overline{z+g+1}^{-\sigma} \times \dots \overline{z+g+l-1}^{-\sigma} \times$ &c. $\times (az^{b} + bz^{b-1} + cz^{b-2} + \&c.)$. If it be required to reduce the term $\overline{rz+f}^{-\rho}$, &c. to a conformity with the reſt, for $\overline{rz+f}^{-\rho}$, &c. ſubſtitute $\overline{z+\frac{f}{r}}^{-\rho} \times r^{-\rho}$, &c. and it is done. Aſſume for the integral or ſum the quantity $S = \overline{z+e}^{-\pi} \cdot \overline{z+e+1}^{-\pi} \dots \overline{z+e+n-2}^{-\pi} \times \overline{rz+f}^{-\rho} \cdot \overline{rz+r+f}^{-\rho} \dots \overline{rz+\overline{m-2}r+f}^{-\rho} \times \overline{z+g}^{-\sigma} \times \overline{z+g+1}^{-\sigma} \dots \overline{z+g+l-2}^{-\sigma} \times$ &c. $\times (\alpha z^{b'} + \beta z^{b'-1} + \&c.) = S$, find its ſucceſſive ſum by writing $z+1$ for z in the ſum S, and let the quantity reſulting be S′; then will the general term be S − S′, which equate to the given general term, that is, their correſpondent quantities; and thence may be deduced the index b' and co-efficients α, β, &c.; and conſequently the ſum ſought. If the ſeries does not terminate, then the ſum will be expreſſed by a ſeries proceeding *in infinitum*, according to the reciprocal dimenſions of z.

From $\pi+\rho+\sigma+$ &c. -1 independent integrals of the above-mentioned kind can be deduced the integrals of all quantities of the ſame kind; that is, where b is any whole affirmative number whatever, and the co-efficients a, b, c, &c. are any how varied.

If any factor $z+g$ in the denominator, &c. has no other $z+g+l-1$, which differs from it by a whole number $l-1$; or the factor $rz+f$ has no correſpondent factor $rz+f+mr$, where m is a whole number; then the integral of the above-mentioned ſeries cannot be expreſſed in finite terms of the quantity z. In like manner, if the dimenſions of z in the numerator are leſs than its dimenſions in the denominator by unity, then the integral of the general term cannot be expreſſed by a finite algebraical function of z. If the number of terms to be added be infinite, it is well known that the ſum in this caſe will be infinite.

It may be obſerved, that in finding the ſum of a ſeries, whoſe general term is given, all common diviſors of the numerator and denominator muſt be rejected, otherwiſe ſerieſes may appear difficult to be ſummed, which are very eaſy: for example, let the ſeries be $\frac{1}{1\cdot 2\cdot 3\cdot 4\cdot 5}+\frac{4}{4\cdot 5\cdot 6\cdot 7\cdot 8}+\frac{9}{7\cdot 8\cdot 9\cdot 10\cdot 11}$ $+\&c. = \frac{1}{3}\left(\frac{1}{1\cdot 2\cdot 4\cdot 5}+\frac{2}{4\cdot 5\cdot 7\cdot 8}+\frac{3}{7\cdot 8\cdot 10\cdot 11}+\&c.\right)$, whoſe general term is $\frac{z+1}{3z+1\cdot 3z+4\times 3z+2\cdot 3z+5}$; and by aſſuming, as s before taught, $\overline{3z+1}^{-1}\times\overline{3z+2}^{-1}\times\alpha$ for the ſum ſought; and finding its general term $\overline{3z+1}^{-1}\times\overline{3z+4}^{-1}\times\overline{3z+2}^{-1}\times$ $\overline{3z+5}^{-1}\times 18\,\overline{z+1}\times\alpha$, which equating to the general term given, there reſults $18\alpha=1$, and the ſum ſought $=\frac{1}{18}\times$ $\frac{1}{3z+1\cdot 3z+2}$.

Ex. 2. Let the ſeries be $\frac{1}{1\cdot 2\cdot 3\cdot 4\cdot 5}+\frac{14}{5\cdot 6\cdot 7\cdot 8\cdot 9}+$ $\frac{55}{9\cdot 10\cdot 11\cdot 12\cdot 13}+\frac{140}{13\cdot 14\cdot 15\cdot 16\cdot 17}+\&c. = \frac{1}{24}\left(\frac{1}{1\cdot 5}+\frac{1}{5\cdot 9}+\right.$ $\left.\frac{1}{9\cdot 13}+\frac{1}{13\cdot 17}+\&c.\right)$, of which the general term is $\frac{1}{24}\times$ $\frac{1}{4z+1\cdot 4z+5}$; and conſequently the ſum deduced is $\frac{1}{24}\times$ $\frac{1}{4}\times\frac{1}{4z+1}$.

Theſe are ſerieſes given by Mr. DE MOIVRE, and eſteemed by Dr. TAYLOR *altioris indaginis.*

Some other writers have made ſome ſerieſes to appear more difficult to be ſummed, by not reducing them to their loweſt terms.

7. Having given the principles of a general method of finding the ſum of a ſeries, when its general term can be expreſſed by algebraical, and not exponential, functions of z, the diſtance from the firſt term of the ſeries; it remains to perform the ſame when exponentials are included.

1. Let S the ſum be any algebraical function of z multiplied into $e^z = x^z$; then will the general term be $Se^z - eS'e^z = (S - eS')\,e^z$; whence, from the general term Te^z being given, aſſume quantities in the ſame manner (with the ſame denominator, &c.) as when no exponential was involved, which multiplied into e^z, ſuppoſe to be the ſum; from the ſum find its general term, and equate it to the given one by equating their correſpondent co-efficients, and it is done.

Ex. Let the general term be $\frac{z+2}{2z+1\,.\,2z+3} \times e^{z+1}$: aſſume for the ſum ſought $\frac{\alpha}{2z+1} \times e^{z+1}$, whence the general term is $\left(\frac{\alpha}{2z+1} - \frac{\alpha e}{2z+3}\right) e^{z+1} = \frac{2\alpha(1-e)z + 3\alpha - \alpha e}{2z+1\,.\,2z+3} \times e^{z+1}$; equate it to the given term, and there reſults $2\alpha(1-e) = 1$ and $3\alpha - \alpha e = 2$, and conſequently $e = \frac{1}{3}$ and $\alpha = \frac{3}{4}$, if the ſeries can be ſummed.

The ſame obſervation, *viz.* that if any factor in the denominator or irrational quantity have no other correſpondent to it; for example, if the factor be $z+g$, and there is no correſpondent one $x+g+n$, where n is a whole number, then its integral cannot be expreſſed by a finite algebraical function of z.

In the ſame manner may the ſums be found, when the terms are exponentials of ſuperior orders; for the exponential, irrational, &c. quantities in the denominators of the ſums may be eaſily deduced from the preceding principles; and thence, by proceeding as is before taught, the ſum required.

The principles of all theſe caſes have been given in the Meditationes.

8. Mr. James Bernoulli found ſummable ſerieſes by aſſuming a ſeries V, whoſe terms at an infinite diſtance are infinitely little, and ſubtracting the ſeries diminiſhed by any number (l) of terms from the ſeries itſelf, &c.

It is obſerved in the Meditationes, that if $T(m)$, $T(m+n)$, $T(m+n+n')$, $T(m+n+n'+n'')$, &c. be the terms at m, $m+n$, $m+n+n'$, $m+n+n'+n''$, &c. diſtances from the firſt, and $aT(m)+bT(m+n)+cT(m+n+n')+dT(m+n+n'+n'')+\&c.$ be the general term, it will be ſummable, when $a+b+c+d+\&c.=0$; the ſum of the ſeries will be $a(T(m)+T(m+1)+T(m+2)+\ldots+T(m+n+n'+n''+\&c.-1))+b(T(m+n)+T(m+n+1)+T(m+n+2)+\ldots+T(m+n+n'+n''+\&c.-1))+c(T(m+n+n')+T(m+n+n'+1)+\ldots(T(m+n+n'+n''+\&c.-1))+\&c.=H$. If the ſum $a+b+c+d+\&c.$ be not $=0$, and the ſeries $T(m)+T(m+1)+T(m+2)+\&c.$ *in infinitum* be a converging one $=S$, then will the ſum of the reſulting ſeries be $(a+b+c+d+\&c.)S-(b+c+d+\&c.)(T^{m}\ldots+T^{m+n-1})-(c+d+\&c.)(T^{m+n}\ldots T^{m+n+n'-1})-(d+\&c.)(T^{m+n+n'}+\ldots T^{m+n+n'+n''-1})-\&c.$

8. 2. Let the ſeries V conſiſt of terms, which have only one factor in the denominator, and its numerator $=1$; that is, let the general term be $\frac{1}{rz+e}$, and the ſeries conſequently $\frac{1}{e}+\frac{1}{r+e}+\frac{1}{2r+e}+\&c.=V$; from the before-mentioned addition or ſubtraction there follows $\frac{a}{rz+e}+\frac{b}{rz+r+e}+\frac{c}{rz+2r+e}+\&c.=$

$\frac{\alpha z^{m}+\beta z^{m-1}+\gamma z^{m-2}+\&c.}{rz+e\,.\,rz+r+e\,.\,rz+2r+e\,.\,\&c.}$; where m is not greater than the number (N) of factors in the denominator diminiſhed by unity. From α, β, γ, &c. r and e being given, eaſily can be acquired by ſimple equations, or known theorems, the required co-efficients a, b, c, &c. If $m=\mathrm{N}-1$ and α and $a+b+c+d+\&c.=0$, then the ſum of the ſeries reſulting will be finite.

8. 3. If the terms of the ſeries aſſumed $\frac{1}{e}-\frac{1}{r+e}+\frac{1}{2r+e}-\frac{1}{3r+e}+\&c.$ be alternately affirmative and negative; then by the preceding caſe find $\frac{\alpha z^{m}+\beta z^{m-1}+\gamma z^{m-2}+\&c.}{rz+e\,.\,rz+r+e\,.\,rz+2r+e+\&c.}=\frac{a}{rz+e}+\frac{b}{rz+r+e}+\frac{c}{rz+2r+e}+\&c.$ Where the terms of the reſulting ſeries are alternately affirmative and negative, let the two ſubſequent terms be ſuppoſed $\frac{\alpha z^{m}+\beta z^{m-1}+\gamma z^{m-2}+\&c.}{rz+e\,.\,rz+r+e\,\ldots\,rz+\overline{n-1}r+e}=\frac{a}{rz+e}+\frac{b}{rz+r+e}+\&c.$ and $\frac{\alpha\overline{z+1}^{m}+\beta\overline{z+1}^{m-1}+\gamma\overline{z+1}^{m-2}+\&c.}{rz+r+e\,.\,rz+2r+e\,\ldots\,rz+nr+e}=\frac{a}{rz+r+e}+\frac{b}{rz+2r+e}+\&c.$ of which the one is affirmative and the other negative: reduce the reſulting ſeries to an affirmative one by ſubtracting the ſubſequent term from its preceding, and it becomes $\frac{(rz+nr+e)\,(\alpha z^{m}+\beta z^{m-1}+\&c.)-(rz+e)\,(\alpha\overline{z+1}^{m}+\beta\overline{z+1}^{m-1}+\&c.)}{rz+e\,.\,rz+r+e\,.\,rz+2r+e\,\ldots\,rz+rn+e}$ $=\frac{\overline{n-m}r\alpha z^{m}+\&c.}{rz+e\,.\,rz+r+e\,..\,rz+rn+e}=\frac{a}{rz+e}+\frac{b-a}{rz+r+e}+\&c.$ In this caſe, ſince two terms are added into one, the diſtance from the firſt term of the ſeries will be $\frac{z}{2}$, which ſuppoſe $=w$; and write $2w$ for z in the above-mentioned term, and there reſults

$$\frac{\overline{n-m}r\alpha z^{m}+\&c.}{rz+e\,.\,rz+r+e\,\ldots\,rz+nr+e}=\frac{\overline{n-m}r\alpha\times2^{m}w^{m}+\&c.}{2rw+e\,.\,2rw+r+e\,\ldots\,2rw+nr+e}=$$

$$\frac{a}{2rw+e}$$

$\frac{a}{2rw+e}+\frac{b-a}{2rw+r+e}+$&c.; whence the ſum of any ſeries, whoſe general term is $\frac{a'w^m+b'w^{m-1}+\&c.}{2rw+e\,.\,2rw+r+e\,\ldots\,2rw+nr+e}$, where m is a whole number leſs than n by two or more, and w the diſtance from the firſt term of the ſeries can be found from the ſum of the ſeries $\frac{1}{e}-\frac{1}{r+e}+\frac{1}{2r+e}-\frac{1}{3r+e}+$&c.

9. Let there be two ſerieſes $\frac{1}{e}+\frac{1}{e+r}+\frac{1}{e+2r}+\&c.=S$ and $\frac{1}{f}+\frac{1}{f+r}+\frac{1}{f+2r}+\frac{1}{f+3r}+\&c.=S'$, whoſe general terms are reſpectively $+\frac{1}{e+rz}$ and $+\frac{1}{f+rz}$; then from the ſum of theſe two ſerieſes can be collected the ſum of any ſeries, whoſe general term is

$$\frac{\alpha z^m+\beta z^{m-1}+\&c.}{rz+e\,.\,rz+e+r\,.\,rz+e+2r\,\ldots\,rz+\overline{n-1}r+e\times rz+f\,.\,rz+r+f\,\ldots\,rz+f+\overline{m-1}r}$$

$$=\frac{a}{rz+e}+\frac{b}{rz+e+r}+\frac{c}{rz+e+2r}\ldots+\frac{\lambda}{rz+\overline{n-1}r+e}+\frac{a'}{rz+f}+\frac{b'}{rz+r+f}$$

$+\frac{c'}{rz+2r+f}\ldots+\frac{\mu'}{rz+\overline{m-1}r+f}$; where $e-f$ is not a whole number. Let $a+b+c\,..+\lambda=0$, and $a'+b'+c'\ldots+\mu'=0$, then the ſum will be $a\left(\frac{1}{rz+e}+\frac{1}{rz+r+e}\ldots+\frac{1}{rz+\overline{n-2}r+e}\right)+b\left(\frac{1}{rz+e+r}\right.$ $\left.+\frac{1}{rz+e+2r}\ldots+\frac{1}{rz+\overline{n-2}r+e}\right)+c\left(\frac{1}{rz+e+2r}+\frac{1}{rz+e+3r}+\,..+\right.$ $\left.\frac{1}{rz+\overline{n-2}r+e}\right)+$&c.$+a'\left(\frac{1}{rz+f}+\frac{1}{rz+r+f}\ldots\frac{1}{rz+\overline{m-2}r+f}\right)+b'$ $\left(\frac{1}{rz+r+f}+\ldots+\frac{1}{rz+\overline{m-2}r+f}\right)+$&c.

2. If the ſerieſes are $\frac{1}{e}-\frac{1}{e+r}+\frac{1}{e+2r}-$&c. and $\frac{1}{f}-\frac{1}{f+r}+\frac{1}{f+2r}$ $-$&c.; then from the ſum of theſe two ſerieſes can be collected by the principles given above the ſum of any ſeries, whoſe general term is

$$\frac{\alpha z^{m}+\beta z^{m-1}+\gamma z^{m-2}+\&c.}{2rz+e \cdot 2rz+r+e \cdot 2rz+2r+e \ldots 2rz+\overline{n-1}r+e \times 2r'z+f \cdot 2r'z+r+f \cdot 2r'z+2r+f \ldots 2r'z+\overline{m-1}r+f}.$$

The ſame principle may be applied to find the ſum of any ſeries of the abovementioned ſort, in whoſe denominator are contained other factors, $rz+g$, $rz+g+r$, &c. &c.; or $2rz+g$, $2rz+g+r$, $2rz+g+2r$, &c. Like propoſitions may be deduced from ſerieſes, in which r and r', &c. and the factors $rz+e$ and $r'z+g$, &c. denote different quantities.

10. An apparently more general method may be given from aſſuming a ſeries or ſerieſes as before; and adding every two, three, four, &c. (n) ſucceſſive terms together for terms of a new ſeries beginning from the firſt, ſecond, third, &c. n^{th} term; and in general adding together two, three, &c. n ſucceſſive general terms; and in their ſum writing for z the diſtance from the firſt term of the ſeries $2z+a$, $3z+a$, &c. $nz+a$; there will reſult the general term of a ſeries not to be found from the above-mentioned addition.

Ex. Let the ſeries aſſumed be $\frac{1}{1}+\frac{1}{2}+\frac{1}{3}+\frac{1}{4}+$ &c. *in infinitum*, of which the general term beginning from the firſt is $\frac{1}{z+1}$; add three ſucceſſive general terms $\frac{1}{z+1}+\frac{1}{z+2}+\frac{1}{z+3}$ $=\frac{3z^2+12z+11}{z+1 \cdot z+2 \cdot z+3}$; in this term for z write $3z$, and there reſults $\frac{27z^2+36z+11}{3z+1 \cdot 3z+2 \cdot 3z+3}$. In the ſame manner, if the beginning is inſtituted from the ſecond or third term of the given ſeries, the terms reſulting will be $\frac{3z^2+18z+26}{z+2 \cdot z+3 \cdot z+4}$ and $\frac{3z^2+24z+47}{z+3 \cdot z+4 \cdot z+5}$. In theſe terms for z write $3z$, and there reſult $\frac{27z^2+54z+26}{3z+2 \cdot 3z+3 \cdot 3z+4}$ and $\frac{27z^2+72z+47}{3z+3 \cdot 3z+4 \cdot 3z+5}$.

If the terms of the given ſeries are alternately affirmative and negative, the terms of the reſulting ſeries will be alternately affirmative and negative, if n be an odd number; otherwiſe its terms will be all affirmative. The ſum of this ſeries will be finite or infinite, as the ſum of the ſeries $1 + \frac{1}{2} + \frac{1}{3} + \frac{1}{4} +$ &c. is finite or infinite; but from it, by the preceding method of addition or ſubtraction of Mr. BERNOULLI's, or a like method applied to more ſerieſes, may be found the ſums of different finite ſerieſes.

It may be obſerved, that from Mr. BERNOULLI's addition or ſubtraction can never be deduced the ſerieſes which ariſe from this method; for, by his method, the denominator can never have any factors but what are contained in the denominators of the given ſeries, *viz.* (in the ſeries $\frac{1}{1} + \frac{1}{2} + \frac{1}{3} +$ &c.), $z + l$, where l is a whole number; but by this method are introduced into the denominator the factors $2z + l$, $3z + l$, &c. and $nz + l$, or which may be reduced to the ſame $\left(z + \frac{l}{n}\right) \times n$.

If n ſucceſſive general terms of the ſerieſes ariſing from Mr. BERNOULLI's addition or ſubtraction be added together, and in the quantity thence ariſing for z the diſtance from the firſt term of the ſeries be ſubſtituted nz, there will be produced ſerieſes of the above-mentioned formula.

11. Multiply two converging ſerieſes $a + bx + cx^2 + dx^3 +$ &c. $= S$ and $\alpha + \beta x + \gamma x^2 +$ &c. $= V$, or find any rational and integral function of them, and the ſeries reſulting will be finite and $= S \times V$, &c. Let $\alpha + \beta x + \gamma x^2 +$ &c. $x^m = V$ be finite, and the reſulting ſeries will be finite and $= S \times V$, &c. If S be a ſeries converging or not, whoſe ultimate terms are leſs than any finite quantity, then will the ſeries $(a + bx + cx^2 +$ &c.$) \times (\alpha + \beta x + \gamma x^2 +$ &c. $x^m) = V \times S$ be a converging one, if $\alpha + \beta x + \gamma x^2 + \ldots$ &c. $x^m = 0$; which caſe was given by Mr. DE MOIVRE.

Mr. BERNOULLI's addition, &c. can be applied to ſerieſes of this kind. For example, let the given ſeries be $\frac{1}{e} + \frac{1}{e+1}x + \frac{1}{e+2}x^2 + \&c. = S$. From this ſeries ſubtract the ſame ſeries diminiſhed by m terms, *viz.* $\frac{1}{e+m}x^m + \frac{1}{e+m+1}x^{m+1} + \frac{1}{e+m+2}x^{m+2} + \&c.$ and there remains $\frac{e+m-ex^m}{e\cdot m+e} + \frac{e+m+1-\overline{e+1}x^m}{e+1\cdot e+m+1}x + \frac{e+m+2-\overline{e+2}x^m}{e+2\cdot e+m+2}x^2 + \frac{e+m+3-\overline{e+3}x^m}{e+3\cdot e+m+3} \times x^3 + \&c.$; for x^m write A, then will the ſeries become $\frac{m-eA}{e\cdot m+e} + \frac{e+m+1-\overline{e+1}A}{e+1\cdot e+m+1}x + \frac{e+m+2-\overline{e+2}A}{e+2\cdot e+m+2}x^2 + \frac{e+m+3-\overline{e+3}A}{e+3\cdot e+m+3}x^3 + \&c. = \frac{1}{e} + \frac{1}{e+1}x + \frac{1}{e+2}x^2 \ldots\ldots \frac{1}{e+m-1}x^{m-1}$.

Let the general term be $\frac{az^m + bz^{m-1} + cz^{m-2} + \&c.}{z+e\cdot z+e+1\cdot z+e+2\ldots z+e+n-1} \times x^z = \left(\frac{\alpha}{z+e} + \frac{\beta}{z+e+1} + \frac{\gamma}{z+e+2} \cdots \frac{X}{z+e+n-1}\right)x^z$. Suppoſe $\beta = \beta' x$, $\gamma = \gamma' x^2$, $\delta = \delta' x^3$, .. $X = X' x^{n-1}$; then will the ſum of the above-mentioned ſeries be $(\alpha + \beta' + \gamma' + \delta' + \&c.) \times S - \frac{1}{e}(\beta' + \gamma' + \delta' + \&c.) - \frac{1}{e+1}(\gamma' + \delta' + \&c.) - \frac{1}{e+2}(\delta' + \&c.) - \&c.$

From the ſum of the ſeries $\frac{1}{e} - \frac{x}{e+1} + \frac{x^2}{e+2} - \&c.$ by theſe and the principles before delivered can be deduced the ſum of any ſeries, whoſe general term is

$$\frac{az^m + bz^{m-1} + \&c.}{2z+e\cdot 2z+e+1\cdot 2z+e+2\cdot 2z+e+3 \times \&c.}x^z.$$

In like manner from the ſum of the ſerieſes $\frac{x}{e} + \frac{x^2}{e+1} + \frac{x^3}{e+2} + \&c.$ $\frac{x}{f} + \frac{x^2}{f+1} + \frac{x^3}{f+2} + \&c.$ $\frac{x}{g} + \frac{x^2}{g+1} + \frac{x^3}{g+2} + \&c.$ &c. can be deduced the ſum of any ſeries, whoſe general term is

$$\frac{az^{m}+bz^{m-1}+\&c.}{z+e\,.\,z+e+1\,.\,z+e+2\times\&c.\times z+f\,.\,z+f+1\,.\,z+f+2\,.\&c.\times z+g\,.\,z+g+1\,.\&c.}\times x^{z}.$$

And alſo from the ſum of the ſerieſes $\frac{1}{e}-\frac{1}{e+1}x+\frac{x^2}{e+2}-$ &c. $\frac{1}{f}-\frac{x}{f+1}+\frac{x^2}{f+2}-$ &c. $\frac{1}{g}-\frac{x}{g+1}+\frac{x^2}{g+2}-$ &c. &c. can be deduced the ſum of any ſeries, whoſe general term is

$$\frac{az^{m}+bz^{m-1}+\&c.}{2z+e\,.\,2z+e+1\,.\,\&c.\times 2z+f\,.\,2z+f+1\,.\,\&c.\,2z+g\,.\,2z+g+1\,.\,\&c.}\times x^{z}.$$

The method of adding more terms of a given ſeries together, as before taught, may be applied to theſe and all other ſerieſes. For example: let the given ſeries be $1+\frac{1}{2}x+\frac{1}{3}x^2+\frac{1}{4}x^3+$ &c.; add two terms conſtantly together, and it becomes $1+\frac{1}{2}x+\&c.=\frac{2+x}{2}+\frac{4+3x}{3\cdot 4}x^2+\frac{6+5x}{5\cdot 6}x^4+\&c.=\frac{2+A}{2}+\frac{4+3A}{3\cdot 4}x^2+\frac{6+5A}{5\cdot 6}x^4+$ &c. whence the general term is $\frac{2z+2+(2z+1)}{2z+2}\frac{x}{2z+1}x^{2z}$. From the methods before given of addition, ſubtraction, and multiplication; and the ſerieſes found by this method, can be derived ſerieſes, whoſe ſums are known.

12. Suppoſe a given ſeries $ax^{n}+bx^{n\pm s}+cx^{n\pm 2s}+dx^{n\pm 3s}+$ &c. whoſe ſum p is either an algebraical, exponential, or fluential fluxion of x; multiply the equation $p=ax^{n}+bx^{n\pm s}+cx^{n\pm 2s}+dx^{n\pm 3s}+$ &c. into $x^{\pm r-n}$, and there reſults $ax^{\pm r-n}p=ax^{\pm r}+bx^{\pm r\pm s}+cx^{\pm r\pm 2s}+$ &c.; find the fluxion of this equation, and there follows $\frac{1}{\dot{x}}$ multiplied into the fluxion of the quantity $(x^{\pm r-n}p)$ $=\pm rax^{\pm r-1}+(\pm r\pm s)\,bx^{\pm r\pm s-1}+(\pm r\pm 2s)\,cx^{\pm r\pm 2s-1}+$ &c. of which the general term is $(\pm r\pm zs)\times t$, where z denotes the diſtance from the firſt term of the ſeries, and t

is the term in the given ſeries, whoſe diſtance from the firſt is z. In the ſame manner may be deduced the ſum of a ſeries whoſe general term is $t' \times \overline{\pm r \pm zs} \times \overline{\pm r' \pm z \pm ns}$, or by repeated operations $t' \times \overline{ez^2 + fz + g}$, where t' is a term of the given equation, whoſe diſtance from the firſt term is z. And in general, from the ſum of a given ſeries, whoſe fluxion can be found, and whoſe general term is t', can be deduced by continued multiplication, and finding the fluxion, the ſum of a ſeries or quantity, of which the general term is At', where A is any function of the following kind $a'z^m + b'z^{m-1} + c'z^{m-2} + \&c.$ in which z denotes the diſtance from the firſt term of the ſeries, and m a whole number. It is to be obſerved, that if the given ſeries converges in a ratio, which is at leaſt equal to the ratio of the convergency of ſome geometrical ſeries, the reſulting equation will always converge. But if in a leſs ratio, then it will ſometimes converge, ſometimes not, according to the ratio which the ſucceſſive terms of the reſulting ſeries have to each other at an infinite diſtance.

Corollary. $\frac{p \cdot p+1 \cdot p+2 \cdot p+3 \cdot\cdot p+z}{r \cdot r+1 \cdot r+2 \cdot r+3 \cdot\cdot r+z} = \frac{p+z \cdot p+z-1 \cdot p+z-2 \cdot p+z-3 \cdot\cdot z+r+1}{r \cdot r+1 \cdot r+2 \cdot r+3 \cdot\cdot\cdot\cdot\cdot p-1}$, if $p - r$ be a whole affirmative number; but this latter quantity has the formula above-mentioned $az^m + bz^{m-2} + cz^{m-3} + \&c.$; and conſequently, if the ſum of the ſeries $a + bx^s + cx^{2s} + dx^{3s} + \&c. = p$ be known, by this method can be deduced the ſum of the ſeries $a + \frac{p}{r} bx^s + \frac{p \cdot p+1}{r \cdot r+1} cx^{2s} + \frac{p \cdot p+1 \cdot p+2}{r \cdot r+1 \cdot r+2} dx^{3s} + \&c.$

Ex. 1. Since $\overline{a+x}^{\frac{m}{n}} = a^{\frac{m}{n}} \left(1 + \frac{m}{n} \times \frac{x}{a} + \frac{m}{n} \times \frac{m-n}{2n} a^{-2}x^2 + \frac{m}{n} \cdot \frac{m-n}{2n} \cdot \frac{m-2n}{3n} a^{-3}x^3 + \&c.\right)$; multiply the ſucceſſive terms of this ſeries into the terms of the ſeries 1, $\frac{p}{r}$, $\frac{p\,.\,\overline{p+1}}{r\,.\,\overline{r+1}}$, &c. and a ſeries is deduced $a^{\frac{m}{n}} + \frac{p}{r\,.\,n}\frac{m}{}a^{+\frac{m}{n}-1}x + \frac{p\,.\,\overline{p+1}\times m\,.\,\overline{m-n}}{r\,.\,\overline{r+1}\,.\,n\,.\,2n}x^{+\frac{m}{n}-2} +$ &c. whoſe ſum is known, if the ſum of the ſeries $= \overline{a+x}^{+\frac{m}{n}}$ is known.

Ex. 2. If the ſeries begins from the $l+1^{\text{th}}$ term of the above-mentioned binomial theorem $a^{\frac{m}{n}} + \frac{m}{n}a^{+\frac{m}{n}-1}x +$ &c. *viz.* the ſeries be $H \times \overline{1 + \frac{m-\overline{l+1}n}{\overline{l+2n}}\frac{x}{a} + \frac{m-\overline{l+2n}}{\overline{l+3n}}\frac{x^2}{a^2} + \frac{m-\overline{l+3n}}{\overline{l+4n}}\frac{x^3}{a^3} + \&c.}$ of which let the reſpective terms be multiplied into 1, $\frac{p}{r}$, $\frac{p\,.\,\overline{p+1}}{r\,.\,\overline{r+1}}$, &c. there will reſult a ſeries whoſe ſum is known.

Ex. 3. From the rule firſt given by me for finding the ſum of the terms at h diſtances from each other, the ſum of the ſeries $1 + \frac{m-\overline{l+1n}}{\overline{l+2}\,.\,n} \times \frac{m-\overline{l+2n}}{\overline{l+3}\,n} \,..\, \frac{m-\overline{l+hn}}{\overline{l+h+1n}} \times \frac{x^h}{a^h} + P \times \frac{m-\overline{l+h+1n}}{\overline{l+h+2n}} \times \frac{m-\overline{l+h+2n}}{\overline{l+h+3n}} \,...\, \frac{m-\overline{l+2hn}}{\overline{l+2h+1n}}\frac{x^{2h}}{a^{2h}} +$ &c. where P denotes the co-efficient of the preceding term, can be deduced; and conſequently the ſum of the ſeries deduced from multiplying the ſucceſſive terms of this ſeries into the quantities 1, $\frac{p}{r}$, $\frac{p\,.\,\overline{p+1}}{r\,.\,\overline{r+1}}$, &c. reſpectively.

The general principles of this caſe were firſt delivered by Mr. Bernoulli, Mr. De Moivre, Mr. Euler, &c.

12. Aſſume the ſeries $a + bx^n + cx^{2n} +$ &c. $= p$, multiply it into $x^{\alpha-1}\dot{x}$, and find the fluent, then will $\frac{1}{\alpha}x^{\alpha}p - \frac{1}{\alpha}\int x^{\alpha}\dot{p} = \frac{1}{\alpha}ax^{\alpha} +$

$\frac{1}{\alpha+n} bx^{\alpha+n} + \frac{1}{\alpha+2n} cx^{\alpha+2n} +$ &c.; multiply this equation into $x^{\beta-\alpha-1}\dot{x}$, and find the fluent of the equation reſulting, which will be $\frac{1}{\beta} \times \frac{1}{\alpha} x^{\beta}p - \frac{1}{\alpha} \cdot \frac{1}{\beta} \int x^{\beta}\dot{p} - \frac{1}{\alpha} \times \frac{1}{\beta-\alpha} x^{\beta-\alpha} \int x^{\alpha}\dot{p} + \frac{1}{\alpha} \cdot \frac{1}{\beta-\alpha} \int x^{\beta}\dot{p} = \frac{1}{\alpha} \cdot \frac{1}{\beta} ax^{\beta} + \frac{1}{\alpha+n} \cdot \frac{1}{\beta+n} bx^{\beta+n} + \frac{1}{\alpha+2n} \cdot \frac{1}{\beta+2n} cx^{\beta+2n} +$ &c.; divide by x^{β}, and there reſults $\frac{1}{\beta} \cdot \frac{1}{\alpha} p + \frac{1}{\alpha} \cdot \frac{1}{\alpha-\beta} x^{-\alpha} \int x^{\alpha}\dot{p} + \frac{1}{\beta} \cdot \frac{1}{\beta-\alpha} x^{-\beta} \int x^{\beta}\dot{p} = \frac{1}{\alpha} \cdot \frac{1}{\beta} a + \frac{1}{\alpha+n} \cdot \frac{1}{\beta+n} bx^{n} +$ &c.: and in general $\frac{1}{\alpha} \cdot \frac{1}{\beta} \cdot \frac{1}{\gamma} \cdot$ &c. $p + \frac{1}{\alpha} \cdot \frac{1}{\alpha-\beta} \cdot \frac{1}{\alpha-\gamma} x^{-\alpha} \int x^{\alpha}\dot{p} + \frac{1}{\beta} \cdot \frac{1}{\beta-\alpha} \cdot \frac{1}{\beta-\gamma} \cdot$ &c. $x^{-\beta} \int x^{\beta}\dot{p} + \frac{1}{\gamma} \cdot \frac{1}{\gamma-\alpha} \cdot \frac{1}{\gamma-\beta}$, &c. $x^{-\gamma} \int x^{\gamma}\dot{p} = \frac{1}{\alpha} \cdot \frac{1}{\beta} \cdot \frac{1}{\gamma}$ &c. $a + \frac{1}{\alpha+n} \cdot \frac{1}{\beta+n} \cdot \frac{1}{\gamma+n}$ &c. $bx^{n} + \frac{1}{\alpha+2n} \cdot \frac{1}{\beta+2n} \cdot \frac{1}{\gamma+2n}$, &c. $cx^{2n} +$ &c. whence the law of continuation is immediately manifeſt.

Hence, if no two quantities α, β, γ, δ, &c. be equal to each other; and the ſucceſſive terms a, b, c, d, &c. of any ſeries $a + bx^{n} + cx^{2n} +$ &c. $= p$ be divided by $\alpha . \beta . \gamma . \delta .$ &c.; $\overline{a+n} . \overline{\beta+n} . \overline{\gamma+n} . \overline{\delta+n} .$ &c.; $\alpha+2n . \beta+2n . \gamma+2n . \delta+2n .$ &c. &c.; and in general by $\alpha+nz . \beta+nz . \gamma+nz . \delta+nz .$ &c. &c.; then can the ſum of the ſeries be found from the fluents of the fluxions $x^{\alpha}\dot{p}$, $x^{\beta}\dot{p}$, $x^{\gamma}\dot{p}$, $x^{\delta}\dot{p}$, &c. as has been obſerved in the Meditationes. If two are equal, *viz.* $\alpha = \beta$, then alſo the fluent of the fluxion $\frac{\dot{x}}{x} \int x^{\alpha}\dot{p}$ is required. If three are equal *viz.* $\alpha = \beta = \gamma$; then it is neceſſary to find the fluent of the fluxion $\frac{\dot{x}}{x} \int \frac{\dot{x}}{x} \int x^{\alpha}\dot{p}$; and ſo on.

1. Let $\dot{p} = \frac{\dot{x}}{1 \pm x^{n}}$; and if the differences of the quantities α, β, γ, δ, &c. are diviſible by n, from the fluent of the

fluxion $x^{\alpha}\dot{p}$ can be deduced the fluents of all the other fluxions $x^{\beta}p$, $x^{\gamma}\dot{p}$, &c.; and in general, if $\alpha-\beta$ is diviſible by n, then from the fluent of the fluxion $x^{\gamma}\dot{p}$ can be deduced the fluent of the fluxion $x^{\delta}\dot{p}$.

2. Suppoſe $p =$ the terms of the binomial theorem expanded according to the dimenſions of x, *viz.* $(a+bx^{n})^{\frac{r}{s}} = a^{\frac{r}{s}} + \frac{r}{s} a^{\frac{r}{s}-1} bx^{n} + \&c.$ beginning from the firſt or any other terms; then, if α, β, &c. divided by n give whole affirmative numbers, will all the fluxions $x^{\alpha}p$, $x^{\beta}p$, $x^{\gamma}p$, &c. be integrable; and if the differences of the quantities α, β, γ, δ, &c. are diviſible by n, from the fluent of the fluxion $x^{\gamma}\dot{p}$ can be deduced the fluents of the fluxions $x^{\beta}\dot{p}$, $x^{\gamma}\dot{p}$, &c.

If p denotes the ſum of the alternate or terms whoſe diſtance from each other are m, of the binomial theorem, the ſame may be applied.

3. If $p=\overline{a+bx^{n}+cx^{2n}}^{\frac{r}{s}}$; and α, β, γ, δ, &c. divided by n give whole affirmative numbers, then from $\int x^{\alpha}\dot{p}$ can be deduced all the remainder $\int x^{\beta}\dot{p}$, $\int x^{\gamma}\dot{p}$, &c.: and in general from two can be deduced all the remainder.

To find when the ſum of any ſeries of this kind can be found, add together each of the fluents, which can be found from each other, and not otherwiſe, and ſuppoſe their ſum $=0$; and ſo of any other ſimilar fluent, and from the reſulting equations can be diſcovered when the ſeries can be integrated.

13. If the general term of a ſeries contains in it more variable quantities, z, v, w, &c.; then find the ſum of the ſeries, firſt, from the hypotheſis that one of them (z) is only variable,

ble, which, properly correcied, let be A; in the quantity A ſuppoſe all the quantities invariable but ſome other v, and find the ſum of the ſeries thence reſulting, which let be B, and ſo on; and the ſum of the ſeries will be deduced.

Ex. Let the term be $\frac{1}{z . z+1 \times v . v+1 . v+2}$; the dimenſions of z and v, &c. in the denominator muſt be at leaſt greater than its dimenſions in the numerator by a quantity greater than the number of the quantities z, v, &c. which proceed *in infinitum* increaſed by unity. Firſt, ſuppoſe z only variable, and the ſum of the infinite ſeries reſulting will be $\frac{1}{z . v . v+1 . v+2} = \text{A}$; then ſuppoſe v only variable, and the ſum reſulting will be $\frac{1}{2z . v . v+1} = \text{B}$, which is the ſum required.

If it be ſuppoſed, that the quantities z and v, &c. in the ſame term ſhall never have the ſame values, then ſuppoſe z and v always to have the ſame values, and the general term $\frac{1}{z . z+1 . v . v+1 . v+2}$ becomes $\frac{1}{z^2 . \overline{z+1}^2 . z+2}$, of which let the ſum be V, then will B − V be the ſum required.

On this and ſome other ſubjects more have been given in the Meditationes.

14. If the ſum of the ſeries cannot be found in finite terms, and it is neceſſary to recur to infinite ſeries; it is obſerved in the Meditationes to be generally neceſſary to add ſo many terms together, that the diſtance from the firſt term of the ſeries may conſiderably exceed the greateſt root of an equation reſulting from the general term made $= 0$; and afterwards a ſeries more converging may commonly be deduced from the fluents of fluxions reſulting from neglecting all but the greateſt quantities in the general terms reſulting; and by other

different methods. Mr. Nicholas Bernoulli and Mr. Mommort inveſtigated the ſum of the ſeries (P) $A + Br + Cr^2 +$ &c. by a ſeries (Q) $\frac{A}{1-r} + \frac{d'r}{(1-r)^2} + \frac{d''r^2}{(1-r)^3} + \frac{d'''r^3}{(1-r)} +$ &c.; where d', d'', d''', &c. denote the ſucceſſive differences of the terms A, B, C, D, &c. If r be negative, the denominators become $1+r$, $(1+r)^2$, $(1+r)^3$, &c.

It has been obſerved, in the Meditationes, that in ſwift converging ſeries the ſeries P will converge more ſwiftly than the ſeries Q; in ſeries converging according to a geometrical ratio, ſometimes the one will converge more ſwift, and ſometimes the other. In other ſeries, which converge more ſlow, where moſt commonly r nearly $= 1$, it cannot in general be ſaid, which of the ſerieſes will converge the ſwifteſt. The preceding remark, *viz.* the addition of the firſt terms of the ſeries, is neceſſary in moſt caſes of finding the ſums by ſerieſes of this kind.

It is not unworthy of obſervation, that in almoſt all caſes of infinite ſeries, the convergency depends on the roots of the given equations, which remark was firſt publiſhed in the Meditationes. For example: in finding approximates to the roots of given equations the convergency depends on how much the approximates given are more near to one root than to any other; and conſequently, when two or more roots or values of an unknown quantity are nearly equal, different rules are to be applied, which are improvements of the rule of falſe. This rule, and the above-mentioned obſervations were firſt given in the Meditationes Algebraicæ et Analyticæ, with ſeveral other additions on ſimilar ſubjects.

Many more things concerning the ſummation of ſeries, which depend on fluxional, &c. equations, might be added; but I ſhall conclude this paper with congratulating myſelf, that ſome algebraical inventions publiſhed by me have been ſince thought not unworthy of being publiſhed by ſome of the greateſt mathematicians of this or any other age.

1ſt, In the year 1757, I ſent to the Royal Society the firſt edition of my Meditationes Algebraicæ: they were printed and publiſhed in the years 1760 and 1762, with Properties of Curve Lines, under the title of Miſcellanea Analytica, and a copy of them ſent to Mr. EULER in the beginning of the year 1763, in which was contained a reſolution of algebraical equations, not inferior, on account of its generality and facility, to any yet publiſhed (*viz.* $y = a\sqrt[n]{p} + b\sqrt[n]{p^2} + c\sqrt[n]{p^3} + \dots \sqrt[n]{p^{n-1}}$). This reſolution was publiſhed by Mr. EULER in the Peterſburg Acts for the year 1764. Whether Mr. EULER ever received my book, I cannot pretend to ſay; nor is it material: for the fact is, that it was publiſhed by me in the year 1760 and 1762, and firſt by Mr. EULER in the year 1764. Mr. DE LA GRANGE and Mr. BEZOUT have aſcribed this reſolution to Mr. EULER, as firſt publiſhed in the year 1764, not having ſeen (I ſuppoſe) my Miſcell. Analyt. Mr. BEZOUT found from it ſome new equations, of which the reſolution is known, and applied it to the reduction of equations: more new equations are given, and the reſolution rendered more eaſy by me in the Philoſophical Tranſactions. 2d, In the above-mentioned Miſcell. Analyt. an equation is transformed into another, of which the roots are the ſquares of the differences of the roots of the given equation; and it is aſſerted in that book, that if the co-efficients co-efficients of the terms of the reſulting equations change continually from + to − and − to +, the roots of the given equation are all poſſible, otherwiſe not; and in a paper, inſerted by me in the Philoſophical Tranſactions for the year 1764, in which is found from this transformation, when there are none, two or four impoſſible roots contained in an algebraical equation of four or five dimenſions; it is obſerved, that there will be none or four, &c. impoſſible roots contained in the given equation, if the laſt term be + or −; and two, &c. on the contrary, if the laſt term be − or +. Theſe obſervations and tranſformation have been ſince publiſhed and explained in the Berlin Acts for the years 1767 and 1768, by Mr. DE LA GRANGE. 3d. In the Miſcell. Anal. an equation is transformed into another, whoſe roots are the ſquares, &c. of the roots of a given equation; and it is aſſerted, that there are at leaſt ſo many impoſſible roots contained in the given equation, as there are continual progreſſes in the reſulting equation from + to + and − to −. It is afterwards remarked, that theſe rules ſometimes find impoſſible roots when Sir ISAAC NEWTON's, and ſuch like rules, fail; and that Sir ISAAC NEWTON's, &c. will find them, when this rule fails. This rule may ſomewhat further be promoted by firſt changing the given equation, whoſe root is x, into another whoſe root is $\sqrt{-1}x$; but, in my opinion, the rule of HARRIOT's, which only finds whether there are impoſſible roots contained in a cubic equation or not, is to be preferred to theſe rules, which, in equations of any dimenſions, of which the impoſſible roots cannot generally be found from the rules, ſeldom find the true number. 4th, It is remarked, that rules which diſcover the true number of impoſſible roots require immenſe calculations, ſince they muſt neceſſarily find, when the roots become equal. In order to this, in the Miſcell. Anal. there is found an equation, whoſe roots are the reciprocals of the differences of any two roots of the given equation; and from finding a quantity (π) greater than the greateſt root of the given, and $\left(\frac{1}{A}\right)$ greater than the greateſt root of the reſulting equation, and ſubſtituting π, $\pi - A$, $\pi - 2A$, &c. for x in the given equation; will always be found the true number of impoſſible roots. 5th, In the ſame book are aſſumed two equations ($nx^{n-1} - \overline{n-1}\,px^{n-2} + \overline{n-2}\,qx^{n-3} -$ &c. $= 0$ and $x^n - px^{n-1} +$ &c. $= w$), and thence deduced an equation, whoſe root is w, from which, in ſome caſes, can be found the number of impoſſible roots.

6. In the Miſcell. Anal. is given the law of a ſeries, and its demonſtration, which finds the ſum of the powers of the roots of a given equation from its co-efficients. Mr. EULER has ſince publiſhed the ſame in the Peterſburg Acts. Mr. DE LA GRANGE printed a property of this ſeries, alſo printed by me about the ſame time; *viz.* that if the ſeries was continued *in infinitum*, the powers would obſerve the ſame law as the roots, which indeed immediately follows from the ſeries itſelf; but from thence with the greateſt ſagacity he deduces the law of the reverſion of the ſeries ($y = a + bx + cx^2 + dx^3 +$ &c.): it has ſince been given in a different manner from ſimilar principles in the Medit. Analyt. 7. In the Miſcell. Analyt. the law of a ſeries is given for finding the ſum of all quantities of this kind ($\alpha^m \times \beta^n \times \gamma^r \times \delta^s \times$ &c. $+$ &c.) where α, β, γ, δ, &c. denote the roots of a given equation, from the powers of the roots of the given equation. This law, with a different notation, has been ſince publiſhed in the Paris Acts by Mr. VANDERMONDE; who indeed mentions that he

had heard, that a ſeries for that purpoſe was contained in my book, but had not ſeen it. In the ſame book is given a method of finding the aggregates of any algebraical functions of each of the roots of given equations, which is ſomewhat improved in the latter editions. 8. In the ſame book are aſſumed $\frac{az^n+bz^{n-1}+\&c.}{pz^m+qz^{m-1}+\&c.}$ and $\frac{Az^{n'}+Bz^{n'-1}+\&c.}{pz^m+qz^{m-1}+\&c.}$, where z is any rational quantity whatever for x and y, the unknown quantities of a given equation of two or more dimenſions. 9. In the Miſcell. Analyt. a biquadratic ($x^4+2px^3=qx^2+rx+s$, of which no term is deſtroyed) is reduced to a quadratic ($x^2+px+n=\sqrt{p^2+2n+q}\,x+\sqrt{s+n^2}$;) and in the ſecond edition of it, printed in the years 1767, 1768, 1769, and publiſhed in the beginning of the year 1770, the values of n are found $\frac{\alpha\beta+\gamma\delta}{2}$, $\frac{\alpha\gamma+\beta\delta}{2}$, and $\frac{\alpha\delta+\beta\gamma}{2}$; and the ſix values of $\sqrt{y^2+2n+q}$ reſpectively $\frac{\alpha+\beta-\gamma-\delta}{2}$, $\frac{\alpha+\gamma-\beta-\delta}{2}$, $\frac{\alpha+\delta-\beta-\gamma}{2}$, and their negatives; and the ſix values of $\sqrt{s+n^2}$ reſpectively $\frac{\alpha\beta-\gamma\delta}{2}$, $\frac{\alpha\gamma-\beta\delta}{2}$, $\frac{\alpha\delta-\beta\gamma}{2}$, and their negatives. 10. From a given biquadratic ($y^4+qy^2+ry+s=0$) by aſſuming $y^2+ay+b=v$ and a and b ſuch quantities as to make the ſecond and fourth terms of the reſulting equations to vaniſh, there reſults an equation ($v^4+Av^2+B=0$) of the formula of a quadratic. Mr. DE LA GRANGE has aſcribed this reſolution to Mr. TSCHIRNHAUSEN; but in the Leipſic Acts the reſolution of a cubic is given by Mr. TSCHIRNHAUSEN, but not of a biquadratic: his general deſign ſeems to be the extermination of all the terms.

11. Mr.

11. Mr. EULER or Mr. DE LA GRANGE found, that if α be a root of the equation $x^n - 1 = 0$, where n is a prime number, α, α^2, α^3, . . α^{n-1}, 1 will be (n) roots of it. More on a ſimilar ſubject has been added in the laſt edition of the Medit. Algebr. 12. It is obſerved in the Miſcell. Analyt. that CARDAN's or SCIPIO FERREUS's reſolution of a cubic is a reſolution of three different cubic equations; and in the Medit. Algeb. 1770, the three cubics are given, and the rationale of the reſolution (for example: if α, β, and γ, be the roots of the cubic equation $x^3 + qx - r = 0$, then is given the function of the above roots, which are the roots of the reducing equation $z^6 - rz^3 = q$); and alſo the rationale of the common reſolution of biquadratics. 13. It is aſſerted in the Miſcell. that if the terms ($My^n + by^{n-1}x + cy^{n-2}x^2 +$ &c. and $Ny^m + By^{m-1}x + Cy^{m-2}x^2 +$ &c.) of two equations of n and m dimenſions, which contain the greateſt dimenſions of x and y have a common diviſor, the equation whoſe root is x or y, will not aſcend to $n \times m$ dimenſions; and if the equation, whoſe root is x or y, aſcends to $n \times m$ dimenſions, the ſum of its roots depends on the terms of n and $n - 1$ dimenſions in the one, and m and $m - 1$ dimenſions in the other equation, &c. It is alſo aſſerted, in the Miſcell. that if three algebraical equations of n, m, and r dimenſions contain three unknown quantities x, y, and z, the equation, whoſe root is x or y or z, cannot aſcend to more than $n \cdot m \cdot r$ dimenſions. 14. Mr. BEZOUT has given two very elegant propoſitions for finding the dimenſions of the equation whoſe root is x or y, &c; where x, y, &c. are unknown quantities contained in two or more (b) algebraical equations of π, ρ, σ, &c. dimenſions, and in which ſome of the unknown quantities do not aſcend to the above π, ρ, σ, &c.

dimenſions reſpectively. In demonſtrating theſe propoſitions he uſes one (amongſt others) before given by me (*viz.* if an equation of n dimenſions contains m unknown quantities, the number of different terms which may be contained in it will be $\overline{n+1} \cdot \frac{n+2}{2} \cdot \frac{n+3}{3} \cdot\cdot \frac{n+m}{m}$). In the Medit. 1770 there is given a method of finding in many caſes the dimenſions of the equation, whoſe root is x or y, &c.; from which one, if not both, of the above-mentioned caſes may more eaſily be deduced, and others added. 15. In the Medit. 1770 is obſerved, that if there be n equations containing m unknown quantities, where n is greater than m, there will be $n-m$ equations of conditions, &c. 16. In the Miſcell. is given and demonſtrated the ſubſequent propoſition; *viz.* if two equations contain two unknown quantities x and y, in which x and y are ſimilarly involved; the equation, whoſe root is x or y will have twice the number of roots which the equation, whoſe root is $x+y$, x^2+y^2, &c. has. In the Medit. 1770 the ſame reaſoning is applied to equations, which have two, three, four, &c. quantities ſimilarly involved. 17. Mr. DE LA GRANGE has done me the honour to demonſtrate my method of finding the number of affirmative and negative roots contained in a biquadratic equation. A demonſtration of my rule for finding the number of affirmative, negative, and impoſſible roots contained in the equation $x^n + Ax^m + B = 0$ is alſo omitted, on account of its eaſe and length. From the Medit. the inveſtigation of finding the true number of affirmative and negative roots appears to be as difficult a problem as the finding the true number of impoſſible roots; and it further appears, that the common methods in both caſes can ſeldom be depended on. But their faults lie on different ſides, the one generally finds too many, the other too few. 18. In the Medit. 1770, from the number of impoſſible roots in a given equation ($x^n - px^{n-1} + \&c. = 0$) is found the number of impoſſible roots in an equation, whoſe roots (v) have any aſſignable relation to the roots of a given equation; and examples are given in the relation ($nx^{n-1} - \overline{n-1}px^{n-2} + \&c. = v$); and in an equation, whoſe roots are the ſquares of the differences of the roots of the given equation. 19. It is obſerved in the Medit. 1770, that in two or more equations, having two or more unknown quantities, the ſame irrationality will be contained in the correſpondent values of each of the unknown quantities, unleſs two or more values of one of them are equal, &c. The ſame obſervation is alſo applied to the coefficients of an equation deduced from a given equation. 20. In the Miſcell. was publiſhed a new method of exterminating, from a given equation, irrational quantities, by finding the the multipliers, which, multiplied into it, give a rational product. 21. In the Medit. 1770, are given the different reſolutions of a certain quantity $(a^2 + rb^2)^{2m+1}$ and $(a^2 + rb^2)^{2m+2}$ into quantities of the ſame kind. 22. Mr. DE LA GRANGE has very elegantly demonſtrated Mr. WILSON's celebrated property of prime numbers contained in my book. In the laſt edition of the Medit. the ſame property is demonſtrated, and ſome ſimilar ones added. 23. In the Miſcell. is given a method of finding all the integral correſpondent values of the unknown quantities of a given ſimple equation, having two or more unknown quantities; and, in the Medit. 1770, are given methods of reducing ſimple and other algebraical equations into one, ſo that ſome unknown quantities may be exterminated; and if the unknown quantities of the reſulting equations be integral or rational, the unknown quantities exterminated may alſo be integral or rational. 24. In the Medit. are given rules for finding the different and correſpondent roots of an equation, whoſe reſolution is given. 25. Mr. DE LA GRANGE has recommended my new transformation of equations, publiſhed in the Miſcell. which perhaps is not leſs general nor elegant than any yet publiſhed; and in the Meditat. 1770 is given a method very uſeful in finding the co-efficients.

If either here, or in the preface to the Medit. Algebraicæ, I have aſcribed to myſelf any algebraical, or in the properties of curve lines any geometrical, or in the Medit. Analyt. any analytical invention, which has been before publiſhed by any other perſon, I can only plead ignorance of it, and ſhall on the very firſt conviction acknowledge it.

I muſt further add, that I have been able to carry my algebraical improvements into geometry; for from them, with ſome geometrical principles added, I have (unleſs I am deceived) deduced as many new properties of conic ſections and curve lines as have been publiſhed by any one ſince the great geometrician APOLLONIUS.

XXIX. *An Account of a remarkable Froſt on the 23d of* June, 1783. *In a Letter from the Rev. Sir* John Cullum, *Bart. F. R. S. and S. A. to Sir* Joſeph Banks, *Bart. P. R. S.*

Read May 27, 1784.

DEAR SIR,

Hardwick-houſe,
Nov. 10, 1783.

WHEN I had the pleaſure of ſeeing you in London, in the autumn, and mentioned a froſt that happened in my neighbourhood on the 23d of laſt June, you expreſſed a deſire of receiving ſome particulars about it. I therefore now ſend you ſome memorandums which I made at the time.

About ſix o'clock, that morning, I obſerved the air very much condenſed in my chamber-window; and, upon getting up, was informed by a tenant, who lives cloſe to my houſe, that finding himſelf cold in bed, about three o'clock in the morning, he looked out at his window, and to his great ſurpriſe ſaw the ground covered with a white froſt: and I was afterwards aſſured, upon indubitable authority, that two men at Barton, about three miles off, ſaw between three and four o'clock that morning, in ſome ſhallow tubs, ice of the thickneſs of a crown-piece, and which was not melted before ſix.

This unſeaſonable froſt produced ſome remarkable effects. The ariſtæ of the barley, which was coming into ear, became brown and withered at their extremities, as did the leaves of the oats; the rye had the appearance of being mildewed; ſo that the farmers were alarmed for thoſe crops. The wheat was not much affected. The larch, Weymouth pine, and hardy Scotch fir, had the tips of their leaves withered; the firſt was particularly damaged, and made a ſhabby appearance the reſt of the ſummer. The leaves of ſome aſhes, very much ſheltered in my garden, ſuffered greatly. A walnut-tree received a ſecond ſhock (the firſt was from a ſevere froſt on the 26th of May) which completed the ruin of its crop. Cherry-trees, a ſtandard peach-tree, filbert and haſel-nut trees, ſhed their leaves plentifully, and littered the walks as in autumn. The barberry-buſh was extremely pinched, as well as the hypericum perforatum and hirſutum: as the two laſt are ſolſtitial, and rather delicate plants, I wondered the leſs at their ſenſibility; but was much ſurpriſed to find, that the vernal black-thorn and ſweet violet, the leaves of which one would have thought muſt have acquired a perfect firmneſs and ſtrength, were injured full as much. All theſe vegetables appeared exactly as if a fire had been lighted near them, that had ſhrivelled and diſcoloured their leaves:

—— *penetrabile frigus adurit.*

At the time this havock was made among ſome of our hardy natives, the exotic mulberry-tree was very little affected; a fig-tree, againſt a north-weſt wall, remained unhurt, as well as the vine, on the other ſide, though juſt coming into bloſſom. I ſpeak of my own garden, which is high; for in the low ones about Bury, that is but a mile off, the fig-trees, in particular, were very much cut: and, in general, all thoſe gardens ſuffer more by froſt than mine.

Some weather, that was cold for the time of year, had preceded this froſt. On the 21ſt the thermometer had, at no time of the day, riſen to 60°; on the 22d, at ten at night had ſunk to 50°. On the laſt day, and on the 23d, diſappeared that dry haze, which had taken place ſome days before, and continued to blot out the face of the ſun for ſo long a time afterwards. After ſun-ſet on the 24th it appeared again, and the next day the leaves of many vegetables were covered with a clammy ſweetneſs.

The above ſlight notes were taken in my garden and its environs; and I wiſh they may afford you the ſmalleſt entertainment. If you ſhould think them worth the attention of the Royal Society, diſpoſe of them accordingly. So ſevere a froſt, at ſo advanced a ſeaſon, is certainly not one of the leaſt remarkable among the atmoſpherical phænomena of this year.

I remain, dear Sir,

Your much obliged and faithful ſervant,

JOHN CULLUM.

XXX. *On a new Method of preparing a Teſt Liquor to ſhew the Preſence of Acids and Alkalies in chemical Mixtures. By Mr.* James Watt, *Engineer; communicated by Sir* Joſeph Banks, *Bart. P. R. S.*

Read May 27, 1784.

THE ſyrop of violets was formerly the teſt of the point of ſaturation of mixtures of acids and alkalies, which was principally uſed; but ſince the late improvements in chemiſtry it has been found not to be ſufficiently accurate, and the infuſion of tourneſol, or of an artificial preparation called litmus, have been ſubſtituted in the place of it.

The infuſion of litmus is blue, and becomes red with acids. It is ſenſible to the preſence of one grain of common oil of vitriol, though it be mixed with 100000 grains of water; but as this infuſion does not change its colour on being mixed with alkaline liquors, in order to diſcover whether a liquor be neutral or alkaline, it is neceſſary to add ſome vinegar to the litmus, ſo as juſt to turn the infuſion red, which will then be reſtored to its blue colour, by being mixed with any alkaline liquor. The blue infuſion of litmus is alſo a teſt of the preſence of fixed air in water, with which it turns red, as it does with other acids.

The great degree of ſenſibility of this teſt would leave very little reaſon to ſearch for any other, were there reaſon to believe that it is always a teſt of the exact point of ſaturation of acids and alkalies, which the following fact ſeems to call in queſtion.

I have obſerved, that a mixture of phlogiſticated nitrous acid with an alkali will appear to be acid, by the teſt of litmus, when other teſts, ſuch as the infuſion of the petals of the ſcarlet roſe, of the blue iris, of violets, and of other flowers, will ſhew the ſame liquor to be alkaline, by turning green ſo very evidently as to leave no doubt.

At the time I made this diſcovery, the ſcarlet roſes and ſeveral other flowers, whoſe petals change their colour by acids and alkalies, were in flower. I ſtained paper with their juices, and found that it was not affected by the phlogiſticated nitrous acid, except in ſo far as it acted the part of a neutralizing acid; but I found alſo, that paper, ſtained in this manner, was by no means ſo eaſily affected by acids of any kind as litmus was, and that in a ſhort time it loſt much of that degree of ſenſibility it poſſeſſed. Having occaſion in winter to repeat ſome experiments, in which the phlogiſticated nitrous acid was concerned, I found my ſtained paper almoſt uſeleſs. I was, therefore, obliged to ſearch for ſome ſubſtitute among the few vegetables which then exiſted in a growing ſtate; of theſe I found the red cabbage (*braſſica rubra*) to furniſh the beſt teſt, and in its freſh ſtate to have more ſenſibility both to acids and alkalies than litmus, and to afford a more deciſive teſt, from its being naturally blue, turning green with alkalies, and red with acids; to which is joined the advantage of its not being affected by phlogiſticated nitrous acid any farther than it acts as a real acid.

To extract the colouring matter, take thoſe leaves of the cabbage, which are freſheſt, and have moſt colour; cut out the larger ſtems, and mince the thin parts of the leaves very ſmall; then digeſt them in water, about the heat of 120 degrees, for a few hours, and they will yield a blue liquor, which, if uſed immediately as a teſt, will be found to poſſeſs great ſenſibility. But, as this liquor is very ſubject to turn acid and putrid, and to loſe its ſenſibility, when it is wanted to be preſerved for future uſe the following proceſſes ſucceed the beſt.

1. After having minced the leaves, ſpread them on paper, and dry them in a gentle heat; when perfectly dry, put them up in glaſs bottles well corked; and when you want to uſe them, acidulate ſome water with vitriolic acid, and digeſt, or infuſe, the dry leaves in it until they give out their colour; then ſtrain the liquor through a cloth, and add to it a quantity of fine whiting or chalk, ſtirring it frequently until it becomes of a true blue colour, neither inclining to green nor purple; as ſoon as you perceive that it has acquired this colour, filter it immediately, otherwiſe it will become greeniſh by longer ſtanding on the whiting.

This liquor will depoſite a ſmall quantity of gypſum, and by the addition of a little ſpirit of wine will keep good for ſome days, after which it will become a little putrid and reddiſh. If too much ſpirit is added, it deſtroys the colour. If the liquor is wanted to be kept longer, it may be neutralized by means of a fixed alkali inſtead of chalk.

2. But as none of theſe means will preſerve the liquor long without requiring to be neutralized afreſh, juſt before it is uſed; and as the putrid and acid fermentation which it undergoes, and perhaps the alkalies or ſpirit of wine mixed with it, ſeem to leſſen its ſenſibility; in order to preſerve its virtues while it is kept in a liquid ſtate, ſome freſh leaves of the cabbage, minced as has been directed, may be infuſed in a mixture of vitriolic acid and water, of about the degree of acidity of vinegar; and it may be neutralized, as it is wanted, either by means of chalk,

or of the fixed or volatile alkali. But it is neceſſary to obſerve, that if the liquor has an exceſs of alkali, it will ſoon loſe its colour, and become yellow, from which ſtate it cannot be reſtored; therefore care ſhould be taken to bring it very exactly to a blue, and not to let it verge towards a green [*].

3. By the ſame proceſs I have made a red infuſion of violets, which, on being neutralized, forms at preſent a very ſenſible teſt; but how long it will preſerve its properties I have not yet determined. Probably the coloured infuſions of other flowers may be preſerved in the ſame manner, by the antiſeptic power of the vitriolic acid, ſo as to loſe little of their original ſenſibility. Paper, freſh ſtained with theſe teſts in their neutral ſtate, has ſufficient ſenſibility for many experiments; but the alum and glue which enter into the preparation of writing-paper ſeem in ſome degree to fix the colour; and paper which is not ſized becomes ſomewhat tranſparent, when wetted, which renders ſmall changes of colour imperceptible; ſo that where accuracy is required, the teſt ſhould be uſed in a liquid ſtate [†].

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[*] Since writing the above, I have found, that the infuſions of red cabbages and of various flowers in water acidulated by means of vitriolic acid, are apt to turn mouldy in the ſummer ſeaſon, and alſo that the moulding is prevented by the addition of ſpirits of wine. The quantity of ſpirit which is neceſſary for this purpoſe I have not been able to aſcertain; but I add it by little at a time, until the progreſs of the moulding is prevented.

[†] I have found, that the petals of the ſcarlet roſe, and thoſe of the pink-coloured lychnis, treated in this manner, afford very ſenſible teſts.

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XXXI. *An Account of a new Plant, of the Order of* Fungi. *By* Thomas Woodward, *Eſq*; *communicated by Sir* Joſeph Banks, *Bart. P. R. S.*

Read June 10, 1784.

Plantæ novæ Deſcriptio — an Genus novum?

Radices paucæ; tenues; albidæ.

Volva ovata; duplex, mucilagine interpoſita; ſubalbida.

Stipes, e volva interiore ſurgens, ſublignoſus; cavus; cortice lacerato veſtitus; ſubfuſcus.

Capitulum, ſtipitis ſummitati inſidens, reflexum; ſubtus campanulatum, glabrum; ſuperne pulverulentum, et, e pulveris craſſitie, globiforme; volvæ ruptæ ſummitatem, minime adhærentem, in ſe gerens.

Pulvis ſphæricus; ſemipellucidus; luteo-fuſcus.

THIS extraordinary vegetable production ariſes from a volva, which is buried ſix or eight inches deep in dry ſandy banks; and, conſequently, it is extremely difficult to detect it in its earlieſt ſtate. At its firſt appearance above ground, the powdery head is covered with a looſe campanulated cap, which does not adhere by any the ſmalleſt filaments; and which, I ſuppoſe, to be the upper part of the volva, as both always appear ragged when taken up. When the plant is taken up immediately on its appearing above ground, the ſtem is about ſix or eight inches long; and, as well as the volva, replete with mucilage, making it much heavier than when it has attained its full growth. This is the ſtate to which the deſcription given above refers. The duſt is now perfectly formed, and is diſperſed by the ſlighteſt touch, or by the wind. A great alteration ſoon takes place, as it now proceeds very rapidly, and in a few days attains the ſummit of its growth, which is from nine to fifteen inches, more than half being generally buried in the ground. The ſtem becomes woody, though hollow, the bark ſtill more ragged, and the whole plant much lighter, both volva and ſtem being now quite dry, and free from mucilage. The wind and ſhowers ſoon diſperſe the greateſt part of the duſt; and at length the ſtalk appears with a naked, coriaceous, campanulated pileus, and conſiderably bleached, in colour and appearance not unlike a dry ſtalk of hemp In this ſtate ſome of them are now to be found (Aug. 28, 1783) with plants of this year riſing near them.

Mr. Humphreys, of Norwich, who firſt found this very extraordinary plant, met with it only in the ſtate laſt deſcribed, and without diſcovering the volva; ſo that no judgement of it could be formed. It has been taken by ſome perſons for a decayed or abortive agaric; but that opinion could not be maintained by any one who had ſeen it in its recent ſtate.

I firſt met with it in February or March 1783 in its dry and withered ſta[illegible] as it was ſuſpected, though with little appearance of [illegible], to be a decayed Agaricus procerus, I wiſhed to examine the root carefully, in order to obſerve whether it was bulbous. The bulb of the Agaricus procerus is ſcarcely hidden under the ſurface, and I was much ſurpriſed at the depth to which I was obliged to ſearch for the root of this plant; at length, however, removing the earth carefully to the depth of ſeven or eight inches, I met with it, and to my great pleaſure and ſurpriſe, on raiſing the plant, I diſcovered the volva, which was ſo unlike the fugitive one of the agaric, that I was immediately convinced it muſt be ſomething new.

An account of this was directly ſent to Mr. DICKSON, of Covent-Garden, an able botaniſt, and diligent enquirer after the claſs Cryptogamia. Mr. DICKSON, who had before ſeen it in the ſtate in which it was found by Mr. HUMPHREYS, but could make nothing of it, though thoroughly convinced it was no agaric, immediately requeſted that I would watch the ſpot, and endeavour to detect the plant in its earlieſt appearance. I communicated this to my neighbour Mr. STONE, a moſt diligent and ſkilful botaniſt, who firſt reſtored the Lycoperdon coliforme; and we determined to examine the ſpot carefully together, from the month of Auguſt downward. About the middle of Auguſt we firſt diſcovered a plant juſt ariſen, which was ſent to Mr. DICKSON, and a full deſcription of which is before given; but though we have daily viſited the ſpot ſince, we have never been able to find it again in ſo young a ſtate; for ſo rapid appears to be its growth, that we have found plants of two or three inches height above the ground, the ſtems of which had loſt part of their mucilage, where the day before none had been viſible. We have three or four times attempted to diſcover the volva in its earlieſt ſtate, by removing the earth carefully near the old ſtems of the preceding year; but this has been without ſucceſs: and there is little hope of ſucceeding in it, as the volva lies very deep in the ground, and the plant ariſes at ſuch various times.

This plant agrees with the genus Phallus in its volva, which has a double coat replete with mucilage; and its ſtipes crowned with a reflexed pileus. But it more nearly approaches the genus Lycoperdon, by its head covered with a thick duſt, contained in a ſubſtance of a ſpongy appearance, and by the form of the duſt, which agrees perfectly with that of moſt of the true lycoperdons, when examined in the microſcope. To this genus it muſt at preſent probably be referred, though the total want of an exterior coat prevents its agreeing with it ſo perfectly as it ought.

The Mucor * ſepticus of Hudson and Lightfoot (Mucor ovatus of Schæffer); the Mucor * butyraceus of Schæffer (194.), not taken notice of by either Hudson or Lightfoot, but which I have often found here; and the Lycoperdon * epidendrum of Lightfoot, which I ſuppoſe to be what Hudson calls Lycoperdon epiphyllum, as he has referred to the ſame plate of Schæffer (193. Mucor fragiformis); have all ſome affinity with the fructification of this plant; and the more ſo, if we ſuppoſe the head to be at firſt covered with a mucilage, which afterwards turns to a duſt; but this will hardly be admitted, as the plant ſent to Mr. Dickson had the duſt perfectly formed, though the volva and ſtem were both replete with mucilage. But we cannot admit it to agree with any of theſe laſt mentioned plants, as they have all an exterior coat, though very fugitive, of which this ſeems entirely deſtitute. We may add, that they are all very fugitive productions;

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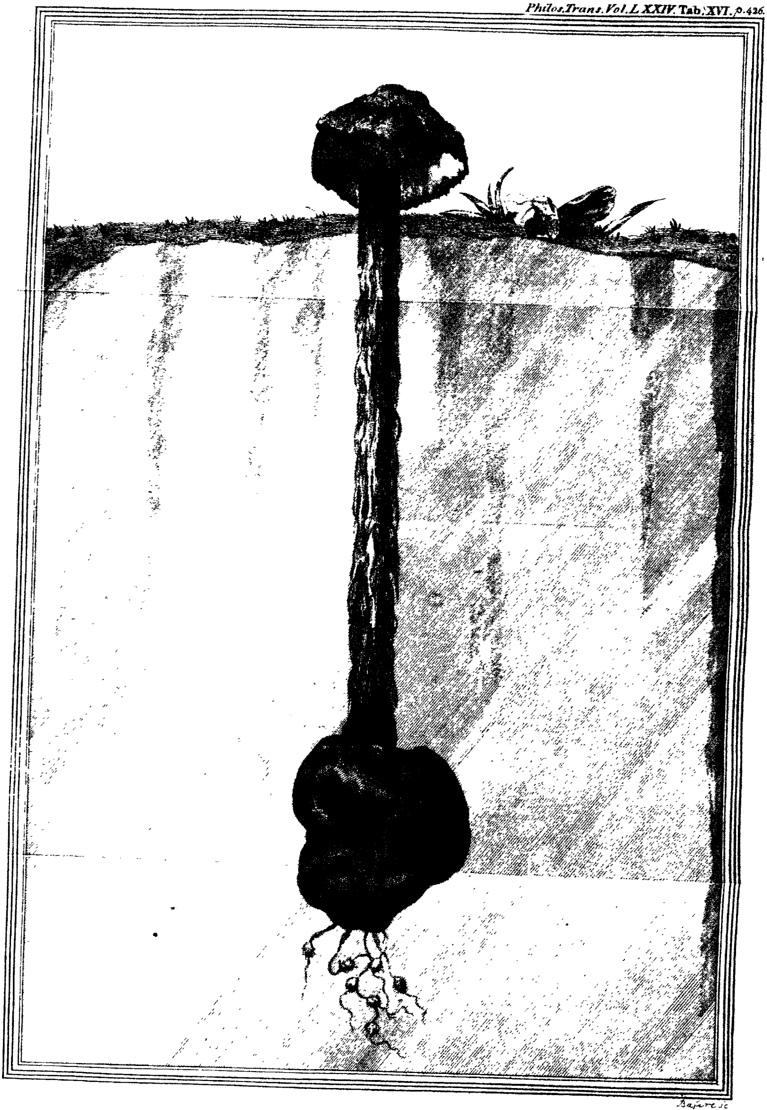
* I cannot help obſerving that, in my opinion, Haller has done more rightly in making theſe into a new genus (Filago), than our botaniſts, who have jumbled them with the genera Lycoperdon and Mucor, to which they have no great affinity any more than the Sphæria of Haller, likewiſe very improperly ranked with the Lycoperdons and Clavariæ.



Basire sc.

whereas this, though ſoon arriving at maturity, is of a woody and permanent ſtructure.

P. S. In a letter Mr. Dickson received from Mr. Woodward, Feb. 12, 1784, he informs him, that he is quite convinced by ſome late obſervations, that the above-mentioned plant frequently comes to a ſtate of perfection before it reaches the ſurface. The only difference to be obſerved is, that the duſt in that caſe is of a darker colour, which he ſuppoſes is owing to its not being expoſed to the air.

XXXII. *Experiments to inveſtigate the Variation of Local Heat.* *By* James Six, *Eſq.*; *communicated by the Rev.* Francis Wollaſton, *LL.B. F. R. S.*

Read June 10, 1784.

BEING deſirous of inveſtigating the variation of local heat, I made the following experiments.

On the 4th of September, 1783, I placed thermometers in three different ſtations; one on the top of the high tower of Canterbury Cathedral, about 220 feet from the ground; another at the bottom of the ſame tower, at about 110 feet; and a third in my own garden *, not more than ſix feet from the ground. They were all carefully expoſed to the open air in a ſhady northern aſpect; the loweſt was as little liable to be affected by the reflection of the ſun's rays as the elevation would permit, the ſecond ſtill leſs, and the higheſt not at all. They continued unremoved in their ſeveral places, where I viſited them daily for the ſpace of three weeks, and minuted down the greateſt degree of heat and cold that happened each day and night in their reſpective ſtations †.

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* This garden is ſituate not far from the Cathedral, at the extremity of the buildings on the north ſide of the city.

† The thermometers here made uſe of were conſtructed to ſhew the greateſt degree of heat and cold which happened in the obſerver's abſence (deſcribed Phil. Tranſ. vol. LXXII. part I.), which rendered them particularly convenient on this occaſion. They had hung together for ſome time, and ſeldom differed half a degree from each other.

By theſe obſervations it appears, ſee Table I. that, notwithſtanding ſome irregularities, the heat of the days at the loweſt ſtation always exceeded that at the middle, and ſtill more the heat at the upper ſtation. As in many inſtances the higher regions of the atmoſphere have been found to be colder than the lower, and the thermometer in the garden was more liable to be heated by the reflection of the ſun's rays from the earth than the upper ones, a difference of this kind might have been expected. But I was greatly ſurpriſed to find the cold of the night at the loweſt, not only equal to, but, very frequently, exceeding the cold at the higher ſtations. As I wiſhed to know, whether theſe variations would continue the ſame in the winter, when the weather was colder; and whether a thermometer, placed at ſome diſtance from the city, having an elevation equal to that on the top of the Cathedral tower, would agree with it; on the 19th of December, 1783, I diſpoſed the three thermometers in the following manner: one in my garden; one on the top of the high tower, as before; and the third on the top of St. Thomas's Hill, about a mile diſtant from the city, where, at fifteen feet from the ground, it was nearly level with that on the Cathedral tower. Table II. contains the obſervations that were then made *. The weather at this time proving cold, favoured the experiment; and I now found the ſeveral thermometers nearly agreeing with each other in the day-time: but in the night, the cold at the lower ſtation exceeded the cold at the higher ones rather more than it did in the month of September, when the weather was warmer.

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* The few omiſſions in this Table were occaſioned by the ſeverity of the cold preventing my attending at a proper time the thermometers, which were at a conſiderable diſtance from each other.

At the time of taking theſe thermometrical obſervations, I likewiſe noted the different diſpoſitions of the atmoſphere in other reſpects: ſuch as the preſſure, moiſture, and dryneſs of the air; force and direction of the winds; quantity of rain; whether the appearances of the ſky were clear or cloudy, &c. as I apprehended the local variation of the thermometers might, in a certain degree, correſpond with ſome particular change in the ſtate of the atmoſphere.

The event anſwered my expectation in a ſingular manner in reſpect to the nocturnal variation; for it generally happened, that when the ſky was dark and cloudy, whatever was the condition of the atmoſphere with relation to the other particulars above enumerated, the thermometers agreed pretty nearly with each other; but, on the contrary, whenever the ſky became clear, the cold of the night at the loweſt ſtation in the garden conſtantly exceeded the cold at the top of the Cathedral tower, where the inſtrument was placed 220 feet from the ground, entirely expoſed to the open air, wind, dews, and rain, in a ſhady northern aſpect.

The local variations in the day-time ſeemed to be regulated by the general degree of heat only, without being affected by any other particular diſpoſition of the atmoſphere, or the clearneſs or cloudyneſs of the ſky, as the nocturnal variations were. In the month of September, when the glaſſes roſe from 60° to 70°, the heat at the lower ſtation conſtantly exceeded the heat at the upper ſtation; and in ſome meaſure proportionally, as the weather was hotter*. In December and January, when from below 30° they ſeldom roſe to 40°, the local variation in the day-time nearly ceaſed, or was found in very ſmall degrees inclining ſometimes one way, ſometimes the other.

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* As the heat at the lower ſtation exceeded the heat at the upper ones, when the weather was hot; and equally ſo, whenever the ſky was cloudy, as well as when it was clear; it appears, that the glaſs at the lower ſtation was not materially affected by the reflection of the ſun's rays from the earth, as at firſt I apprehended it would be.

That the clearneſs of the ſky ſhould contribute to the coolneſs of the air in the night, is not at all ſurpriſing; but that, whenever the ſky becomes clear, the cold ſhould ſeem to ariſe from the earth, and be found in the greateſt degree, as long as it continues clear, in the loweſt ſituation, ſeems a little extraordinary: this, however, appeared to be the caſe, both in the warmer as well as in the colder weather, during the whole time theſe obſervations were taken, and remarkably ſo on the following days. On the firſt of January the weather was cold, the ſky cloudy, the glaſſes in the night were at 20°, and in the day at 34°: the wind which had been at S.E. the day before, changed in the evening to S. and brought on a thaw. On the ſecond of January clouds and miſty rain darkened the ſky all day; the wind blew briſkly at S.W.; the glaſſes in the night were at 32°½, in the day at 40°. On the third of January the clouds and rain continued, the weather growing ſtill warmer; wind at S.W. by S.; the glaſſes in the night were at 36°, in the day at 45°½. Theſe three days the weather gradually became warmer; and, while the ſky remained darkened by clouds, all the glaſſes in their ſeveral ſtations nearly agreed with each other. About noon, on the third of January, the ſky becoming clear, the air grew cooler; and going into my garden, about eight o'clock in the evening, I perceived the ſurface of the ground, which had been wet by the rain in the forenoon, began to be frozen. Looking immediately at the thermometer, I ſaw the mercury at 33°½; and obſerving a piece of wet linen hanging near the glaſs, not five feet from the ground, I took it into my hand, and found it not in the leaſt frozen; by which it appeared, that the degree of cold which had frozen the ſurface of the ground, had not then aſcended to the glaſs, nor to the linen, and conſequently had not been communicated to the air five or ſix feet above the earth. The next day I found, as I expected, a conſiderable local variation; the index for the cold of the night in the garden being at 32°, that on the hill being at 35°¼, and that on the top of the tower at 37°¾ *. Probably the weather did not continue clear the whole night; if it had, it is likely the degrees of cold would have been found proportionally greater at every ſtation. On the morning of the 4th there fell a miſty rain, which continued only till noon, when the ſky became clear again, and continued ſo till the 7th; during which time the nocturnal heights of the thermometers differed conſiderably from each other; but on the ſky's becoming cloudy, the local variation ceaſed.

Thermometrical obſervations, made under the ſame circumſtances in reſpect to the ſeaſon of the year, place, and ſituation †, may probably be liable to ſimilar local varia-

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* It is remarkable, that the thermometer on St. Thomas's hill did not vary ſo much from that in the garden, as that did which was on the Cathedral tower, although theſe two elevated glaſſes were within three feet of a perfect level with each other; the variations, however, as often as they happened, inclined the ſame way. The reaſon of this might probably be, that although the glaſs on the hill was at an equal altitude with that on the tower, in reſpect to the ground on which the Cathedral ſtands: yet the former was only 15 feet, while the latter was 220 feet from the ground.

† Situation in regard to hill or valley. The valley in which Canterbury ſtands is at that place about a mile in breadth, opening to the N.E.; the hills on either ſide do not riſe very ſudden, nor very high; the river Stour, divided into branches, paſſes through the city, and, about fourteen miles below, empties itſelf into the ſea, which waſhes the coaſt from the NN.W. round by the E. to the S.; diſtant from the city at different places from ſix to ſixteen miles.

tions: to thoſe who make them, the reſult of theſe experiments may be of ſome uſe. If convenient opportunity offered, I ſhould be glad, by the aſſiſtance of friends, to try the local difference of heat and cold in more diſtant, as well as more elevated, ſituations.

By experiments of this kind it may poſſibly in ſome meaſure be found, how far evaporations from the earth, at certain times, or vapours aſcending, deſcending, or meeting, in different parts of the atmoſphere, may increaſe or diminiſh the heat of the air in thoſe places: or whether different degrees of heat and cold (ſubject however to change) may not be found in different ſtrata of air, or vapour, floating in different parts of the atmoſphere; or in what degree and proportion, the cold increaſes at different altitudes and in different ſeaſons of the year: whether the cold, which is known to be very intenſe in the ſummer time on the tops of high mountains, receives a proportional increaſe, or be not leſs ſubject to variety by the return of winter and ſummer, night and day, than what we experience in the plains below.

March 10, 1784. JAMES SIX.

TABLE I.

The greateſt daily variation of heat and cold in the atmoſphere, from the 4th to the 24th of September, 1783, taken from three different ſtations, and compared together. One thermometer placed on a tower in Canterbury, 220 feet from the ground; another at the bottom of the ſame tower, 110; and a third in a garden, about ſix feet from the ground. N. B. The nocturnal degrees of cold belong to the night immediately preceding the day to the date of which they are placed.

	Greateſt degree of cold in the night.					Greateſt degree of heat in the day.					
	Thermometer in the garden.	Thermometer at the bottom of the tower.	Thermometer on the top of the tower.	Difference of garden from bottom of tower.	Difference of garden from top of tower.	Thermometer in the garden.	Thermometer at the bottom of the tower.	Thermometer on the top of the tower.	Difference of garden from bottom of tower.	Difference of garden from top of tower.	
Sept. 4	50½°	—°	51°	—°	−0½°	66°	61°	61°	+5°	+5°	Morning ſtill and foggy; wind began to blow in the forenoon at S.W.; clouds and rain in the afternoon and night; bar. 29.3.
5	48	47¼	47½	+0¾	+0½	62½	61½	61½	+1	+1	Morning cloudy; heavy rain; clear in the afternoon; wind high at W.N.W.; bar. 29.3.
6	48½	50	50½	−1½	−2	66½	65¼	64½	+1¼	+2	Morning rained a little; wind very high at S.W. moſt part of the day; bar. 29.5.
7	48	49½	49½	−1½	−1½	63½	62½	63	+1	+0½	Morning clear; continued ſo moſt part of the day; wind very high at W.; bar. 29.8.
8	50	51	51	−1	−1	66	62½	62	+3½	+4	Sometimes clear, ſometimes cloudy; wind very high at W.; bar. 29.9.
9	55½	55½	55½	—	—	65	63	62½	+2	+2½	Morning cloſe and cloudy; clear at noon; wind briſk at S.W.; bar. 29.5.
10	45	47	47½	2	−2½	63¾	59¾	59¾	+4	+4	Morning and great part of the day clear; wind high at S.W.; evening clear; bar. 29.6.
11	42	45	45½	−3	−3½	63½	62	60½	+1½	+3	Morning clear; cloudy about noon; briſk wind at S.; evening ſtill; bar. 29.8.
12	52½	53½	54	−1	−1½	69	66½	65	+2½	+4	Morning cloudy; wind high at S.; evening ſtill and clear; bar. 29.4.
13	45	48	48½	−3	−3½	65	62	62	+3	+3	Morning clear: a little rain at noon; cloudy afternoon; wind briſk at S.; bar. 29.0.
14	57½	57	57	+0½	+0½	68½	66½	64½	+2	+4	Morning cloudy; moiſt warm air; wind briſk at S.W.; miſty rain; cloudy evening; bar. 29.8.
15	57	57	58	—	−1	70	68½	66	+1½	+4	Morning cloudy; wind moderate S.W.; in the evening changed to N.; bar. 29.5.
16	52½	53	52½	−0½	—	65½	62½	61	+3	+4½	Morning hazy; thin clouds all day; little wind at N.E.; cloſe and warm; bar. 29.8.
17	51½	51½	51	—	+0½	62½	61	60½	+1½	+2	Dull and hazy moſt part of the day; little breeze of wind at N.E.; bar. 30.1.
18	57	57	57	—	—	62½	62	61	+0½	+1½	Very dull all day; wind briſk at N.E.; bar 29.9.
19	53	54½	55½	−1½	−2½	70	67½	67	+2½	+3	Morning clear; a little rain in the afternoon; wind S.S.E.; bar. 29.6.
20	56	56	55½	—	+0½	64½	63	61	+1½	+3½	Cloudy all day, with rain and wind S.W.; clear at night, bar. 29 4.
21	44½	47½	48½	−3	−4	63½	61	60½	+2½	+3	Morning clear, wind at S.W.; clear moſt part of the day; wind S.; bar. 29.8.
22	56	57	57	−1	−1	59	58	58	+1	+1	Rain moſt part of the day; evening hazy; wind S.W.; bar. 29.6.
23	50	49	50	+1	—	63	59½	59½	+3½	+3½	Morning ſtill and miſty; a little ſhower in the afternoon, clear all the reſt of the day; wind S.W.; bar. 29.6.
24	43¾	46	46	−2¼	−2¼	63	59¾	58¾	+3¼	+4¼	Clear all day; wind W. and N.W.; bar. 29.8.

TABLE II.

The greateſt daily variation of heat and cold in the atmoſphere from the 20th of December, 1783, to the 8th of January, 1784, taken from three different ſtations, and compared together. One thermometer placed on a tower in Canterbury, 220 feet from the ground; another on a hill, a mile diſtant, but on the ſame level with that on the tower; a third in a garden, about ſix feet from the ground. N. B. The nocturnal degrees of cold belong to the night immediately preceding the day to the date of which they are placed.

	Greateſt degree of cold in the night					Greateſt degree of heat in the day.					
	Thermometer in the garden.	Thermometer on the hill.	Thermometer on the tower.	Difference of garden from hill.	Difference of garden from tower.	Thermometer in the garden.	Thermometer on the hill.	Thermometer on the tower.	Difference of garden from hill	Difference of garden from tower.	
Dec. 20	20°	25°	25¾°	−5°	− 5¾°	39¼°	37¾°	39¾°	+ 1½°	−0½°	Evening preceding clear; morning clear; wind briſk at W.; cloudy at noon; air very moiſt; barometer at 29.9.
21	29¾	30¾	32	− 1	− 2¼	37¼	38½	38	− 0¾	− 0¼	Morning clear on the hill; fog in the city; little wind at N.W.; air moiſt: cloudy at noon; a little ſnow; bar. 29.8.
22	22	24½	25½	− 2½	− 3½	34	34¾	36	− 0¾	− 2	Morning ſlight fog in the city; little wind at N.W.; air moiſt; cloudy at noon; a little ſnow; bar. 29.7.
23	31¾	31¾	32	——	− 0¼	39¾	40¼	37½	− 0½	+ 2¼	Dark and cloudy all day; wind briſk at S.W.; air moiſt; evening foggy; bar. 29.8.
24	31¾	33	34½	− 1¼	− 2¾	43¾	42	41¼	+ 1¾	+ 2½	Morning rainy; clear at noon; cloudy late in the evening; wind at N.; bar. 29.6.
25	26	27	—	− 1	——	36	35¼	—	+ 0¾	——	Morning cloudy; wet miſt; wind briſk at E., towards evening rain mixed with ſnow; night clear; bar. 29.2.
26	26½	26½	28	——	− 1½	33½	34	36	− 0½	− 2½	Morning foggy; a little ſnow about noon; evening clear in the zenith; little wind at N.; bar. 28.9.
27	25	26½	28	− 1½	− 3	36	34¾	35½	+ 1¼	− 0½	Morning clear; cloudy at noon; little ſnow in the evening; moderate breeze of wind at N.E.; bar. 29.3.
28	30	29	29½	+ 1	+ 0½	31¼	30	33½	+ 1¼	− 2¼	Morning dark and cloudy; wind very briſk at N.E.; air dry, and felt very cold; bar. 29.3.
29	21	21½	22	− 0½	− 1	24½	24¾	—	− 0¼	——	Morning hazy; dry miſty air; wind very cold and briſk at S.E. by E.; bar. 29.7.
30	15½	15	16½	+ 0½	− 1	22	21½	21½	+ 0½	+ 0½	Morning clear in the zenith; dry miſty fog below; wind very cold and briſk at S.E. bar. 29.7.
31	12¼	11¾	13	+ 0½	− 0¾	21½	—	21½	——	——	Sometimes clear, ſometimes cloudy; wind briſk at S.E.; evening rain with ſnow; wind S. very high in the night; bar. 29.4.
Jan. 1	20	—	20	——	——	34	—	33½	——	+ 0½	Wet miſt all day; wind moderate at W.; bar. 29.4.
2	32½	32	32½	+ 0½	——	40	39½	40	+ 0½	——	Morning thick fog and miſty rain; wind S.E.; afternoon and evening very high at S.W. with rain; bar. 29.6.
3	36	35½	36	+ 0½	——	45½	45	45½	+ 0½	——	Morning rainy; wind S.W. by W.; clear at noon; afternoon and evening very clear and ſtill; wind S.W.; bar. 29.6.
4	32	35¾	37¾	− 3¾	− 5¾	46½	44¾	45½	+ 1¾	+ 1	Morning miſty rain; wind high at S.; evening very clear and ſtill; bar. 29.8.
5	26¾	29¾	31	− 3	− 4¼	36½	35½	—	+ 1	——	Morning very clear; little wind at N.E evening very clear and ſtill; bar. 30.1.
6	21½	26	27½	− 4½	− 6	31	30¾	—	+ 0¼	——	Morning very clear. moderate breeze of wind at SE.; evening very clear and ſtill; bar. 30.2.
7	16	19	20½	− 3	− 4½	29	27½	27½	+ 1½	+ 1½	Morning very clear; cloudy about noon, evening dark and cloudy; little wind at S.E.; bar. 30.0.
8	26	25½	25½	+ 0½	+ 0½	32	32	31½	——	+ 0½	Morning dark and cloſe; very dark all day; wind W.N W.; bar. 29.8.

* * *

XXXIII. *Account of ſome Obſervations tending to inveſtigate the Conſtruction of the Heavens.* *By* William Herſchel, *Eſq.* *F. R. S.*

Read June 17, 1784.

IN a former paper I mentioned, that a more powerful inſtrument was preparing for continuing my reviews of the heavens. The teleſcope I have lately completed, though far inferior in ſize to the one I had undertaken to conſtruct when that paper was written, is of the Newtonian form, the object ſpeculum being of 20 feet focal length, and its aperture $18\frac{7}{10}$ inches. The apparatus on which it is mounted is contrived ſo as at preſent to confine the inſtrument to a meridional ſituation, and by its motions to give the right-aſcenſion and declination of a celeſtial object in a coarſe way; which, however, is ſufficiently accurate to point out the place of the object, ſo that it may be found again. It will not be neceſſary to enter into a more particular deſcription of the apparatus, ſince the account I have now the honour of communicating to the Royal Society regards rather the performance of the teleſcope than its conſtruction.

It would, perhaps, have been more eligible to have waited longer, in order to complete the diſcoveries that ſeem to lie within the reach of this inſtrument, and are already, in ſome reſpects, pointed out to me by it. By taking more time I ould undoubtedly be enabled to ſpeak more confidently of the

interior conſtruction of the heavens, and its various ***nebulous and ſidereal ſtrata*** (to borrow a term from the natural hiſtorian) of which this paper can as yet only give a few outlines, or rather hints. As an apology, however, for this prematurity, it may be ſaid, that the end of all diſcoveries being communication, we can never be too ready in giving facts and obſervations, whatever we may be in reaſoning upon them.

Hitherto the ſidereal heavens have, not inadequately for the purpoſe deſigned, been repreſented by the concave ſurface of a ſphere, in the center of which the eye of an obſerver might be ſuppoſed to be placed. It is true, the various magnitudes of the fixed ſtars even then plainly ſuggeſted to us, and would have better ſuited the idea of an expanded firmament of three dimenſions; but the obſervations upon which I am now going to enter ſtill farther illuſtrate and enforce the neceſſity of conſidering the heavens in this point of view. In future, therefore, we ſhall look upon thoſe regions into which we may now penetrate by means of ſuch large teleſcopes, as a naturaliſt regards a rich extent of ground or chain of mountains, containing ſtrata variouſly inclined and directed, as well as conſiſting of very different materials. A ſurface of a globe or map, therefore, will but ill delineate the interior parts of the heavens.

It may well be expected, that the great advantage of a large aperture would be moſt ſenſibly perceived with all thoſe objects that require much light, ſuch as the very ſmall and immenſely diſtant fixed ſtars, the very faint nebulæ, the cloſe and compreſſed cluſters of ſtars, and the remote planets.

On applying the teleſcope to a part of the *via lactea*, I found that it completely reſolved the whole whitiſh appearance into ſmall ſtars, which my former teleſcopes had not light enough to effect. The portion of this extenſive tract, which it has hitherto been convenient for me to obſerve, is that immediately about the hand and club of Orion. The glorious multitude of ſtars of all poſſible ſizes that preſented themſelves here to my view was truly aſtoniſhing; but, as the dazzling brightneſs of glittering ſtars may eaſily miſlead us ſo far as to eſtimate their number greater than it really is, I endeavoured to aſcertain this point by counting many fields, and computing, from a mean of them, what a certain given portion of the milky way might contain. Among many trials of this ſort I found, laſt January the 18th, that ſix fields, promiſcuouſly taken, contained 110, 60, 70, 90, 70, and 74 ſtars each. I then tried to pick out the moſt vacant place that was to be found in that neighbourhood, and counted 63 ſtars. A mean of the firſt ſix gives 79 ſtars for each field. Hence, by allowing 15 minutes of a great circle for the diameter of my field of view, we gather, that a belt of 15 degrees long and two broad, or the quantity which I have often ſeen paſs through the field of my teleſcope in one hour's time, could not well contain leſs than fifty thouſand ſtars, that were large enough to be diſtinctly numbered. But, beſides theſe, I ſuſpected at leaſt twice as many more, which, for want of light, I could only ſee now and then by faint glittering and interrupted glimpſes.

The excellent collection of nebulæ and cluſters of ſtars which has lately been given in the *Connoiſſance des Temps* for 1783 and 1784, leads me next to a ſubject which, indeed, muſt open a new view of the heavens. As ſoon as the firſt of theſe volumes came to my hands, I applied my former 20-feet reflector of 12 inches aperture to them; and ſaw, with the greateſt pleaſure, that moſt of the nebulæ, which I had an opportunity of examining in proper ſituations, yielded to the force of my light and power, and were reſolved into ſtars. For inſtance, the 2d, 5, 9, 10, 12, 13, 14, 15, 16, 19, 22, 24, 28, 30, 31, 37, 51, 52, 53, 55, 56, 62, 65, 66, 67, 71, 72, 74, 92, all which are ſaid to be nebulæ without ſtars, have either plainly appeared to be nothing but ſtars, or at leaſt to contain ſtars, and to ſhew every other indication of conſiſting of them entirely. I have examined them with a careful ſcrutiny of various powers and light, and generally in the meridian. I ſhould mention, that five of the above, *viz.* the 16th, 24, 37, 52, 67, are called cluſters of ſtars containing nebuloſity; but my inſtrument reſolving alſo that portion of them, which is called nebulous into ſtars of a much ſmaller ſize, I have placed them into the above number. To theſe may be added the 1ſt, 3d, 27, 33, 57, 79, 81, 82, 101, which in my 7, 10, and 20-feet reflectors ſhewed a mottled kind of nebuloſity, which I ſhall call reſolvable; ſo that I expect my preſent teleſcope will, perhaps, render the ſtars viſible of which I ſuppoſe them to be compoſed. Here I might point out many precautions neceſſary to be taken with the very beſt inſtruments, in order to ſucceed in the reſolution of the moſt difficult of them; but reſerving this at preſent too extenſive ſubject for a future opportunity, I proceed to ſpeak of the effects of my laſt inſtrument with regard to nebulæ.

My preſent purſuits, as I obſerved before, requiring this teleſcope to act as a fixed inſtrument, I found it not convenient to apply it to any other of the nebulæ in the *Connoiſſance des Temps* but ſuch as came in turn; nor, indeed, was it neceſſary to take any particular pains to look for them, it being utterly impoſſible that any one of them ſhould eſcape my obſervation when it paſſed the field of view of my teleſcope. The few which I have already had an opportunity of examining, ſhew plainly that thoſe moſt excellent French aſtronomers, Meſſ. Messier and Mechain, ſaw only the more luminous part of their nebulæ; the feeble ſhape of the remainder, for want of light, eſcaping their notice. The difference will appear when we compare my obſervation of the 98th nebula with that in the *Connoiſſance des Temps* for 1784, which runs thus: "Nébuleuſe ſans étoile, "d'une lumière extrêmement foible, au deſſus de l'aile boréale "de la Vierge, ſur le parallèle et près de l'étoile N° 6. cin-"quième grandeur, de la chevelure de Bérénice, ſuivant "Flamsteed. M. Mechain la vit le 15 Mars, 1781." My obſervation of the 30th of December, 1783, is thus: A large, extended, fine nebula. Its ſituation ſhews it to be M. Messier's 98th; but from the deſcription it appears, that that gentleman has not ſeen the whole of it, for its feeble branches extend above a quarter of a degree, of which no notice is taken. Near the middle of it are a few ſtars viſible, and more ſuſpected My field of view will not quite take in the whole nebula. See fig. 1. tab. XVII. Again, N° 53. "Nébuleuſe ſans étoiles, "decouverte au-deſſous et près de la chevelure de Bérénice, à "peu de diſtance de l'étoile quarante-deuxieme de cette conſtel-"lation, ſuivant Flamsteed. Cette nébuleuſe eſt ronde et "apparente, &c." My obſervation of the 170th Sweep runs thus: A cluſter of very cloſe ſtars; one of the moſt beautiful objects I remember to have ſeen in the heavens. The cluſter appears under the form of a ſolid ball, conſiſting of ſmall ſtars, quite compreſſed into one blaze of light, with a great number of looſe ones ſurrounding it, and diſtinctly viſible in the general maſs. See fig. 2.

When I began my preſent ſeries of obſervations, I ſurmiſed, that ſeveral nebulæ might yet remain undiſcovered, for want of ſufficient light to detect them; and was, therefore, in hopes of making a valuable addition to the cluſters of ſtars and nebulæ already collected and given us in the work before referred to, which amount to 103. The event has plainly proved that my expectations were well founded: for I have already found 466 new nebulæ and cluſters of ſtars, none of which, to my preſent knowledge, have been ſeen before by any perſon; moſt of them, indeed, are not within the reach of the beſt common teleſcopes now in uſe. In all probability many more are ſtill in reſerve; and as I am purſuing this track, I ſhall make them up into ſeparate catalogues, of about two or three hundred at a time, and have the honour of preſenting them in that form to the Royal Society.

A very remarkable circumſtance attending the nebulæ and cluſters of ſtars is, that they are arranged into ſtrata, which ſeem to run on to a great length; and ſome of them I have already been able to purſue, ſo as to gueſs pretty well at their form and direction. It is probable enough, that they may ſurround the whole apparent ſphere of the heavens, not unlike the milky way, which undoubtedly is nothing but a ſtratum of fixed ſtars. And as this latter immenſe ſtarry bed is not of equal breadth or luſtre in every part, nor runs on in one ſtraight direction, but is curved and even divided into two ſtreams along a very conſiderable portion of it; we may likewiſe expect the greateſt variety in the ſtrata of the cluſters of ſtars and nebulæ. One of theſe nebulous beds is ſo rich, that, in paſſing through a ſection of it, in the time of only 36 minutes, I detected no leſs than 31 nebulæ, all diſtinctly viſible upon a fine blue ſky. Their ſituation and ſhape, as well as condition, ſeems to denote the greateſt variety imaginable. In another ſtratum, or perhaps a different branch of the former, I have ſeen double and treble nebulæ, variouſly arranged; large ones

Fig. 1.

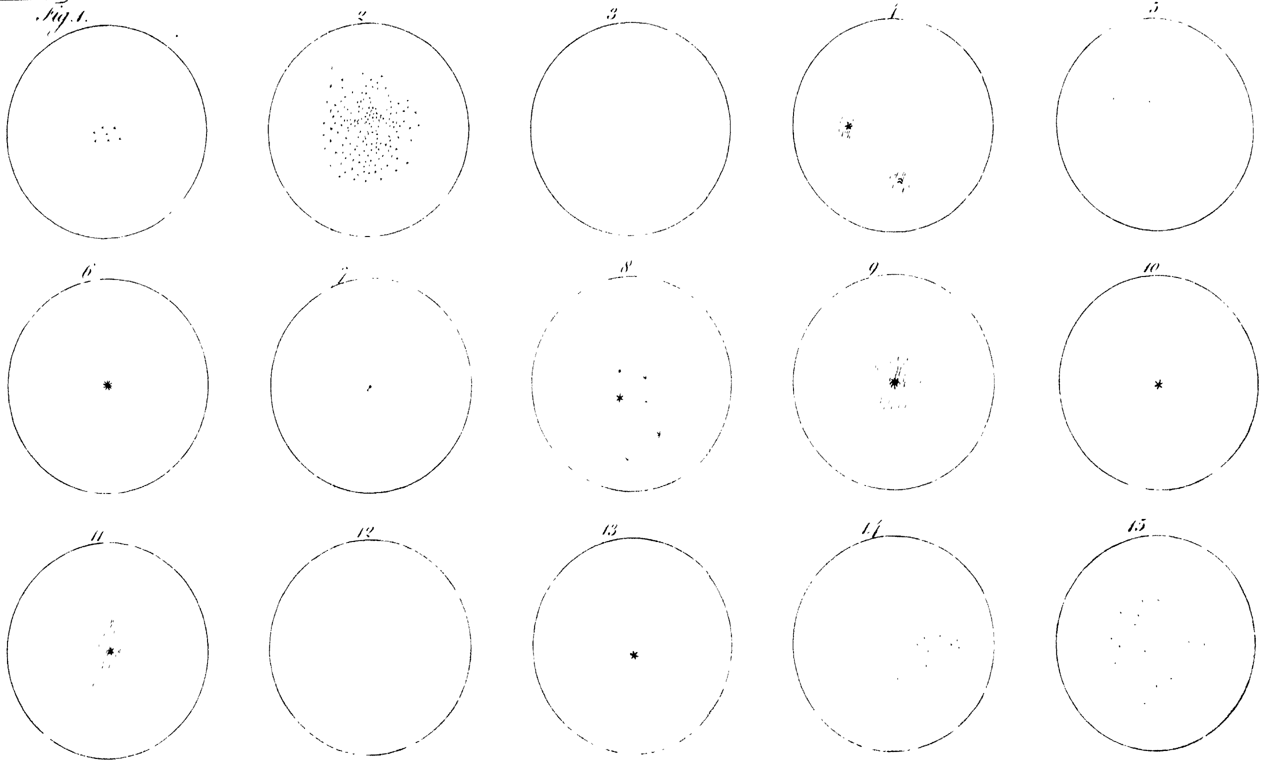


Fig. 16.

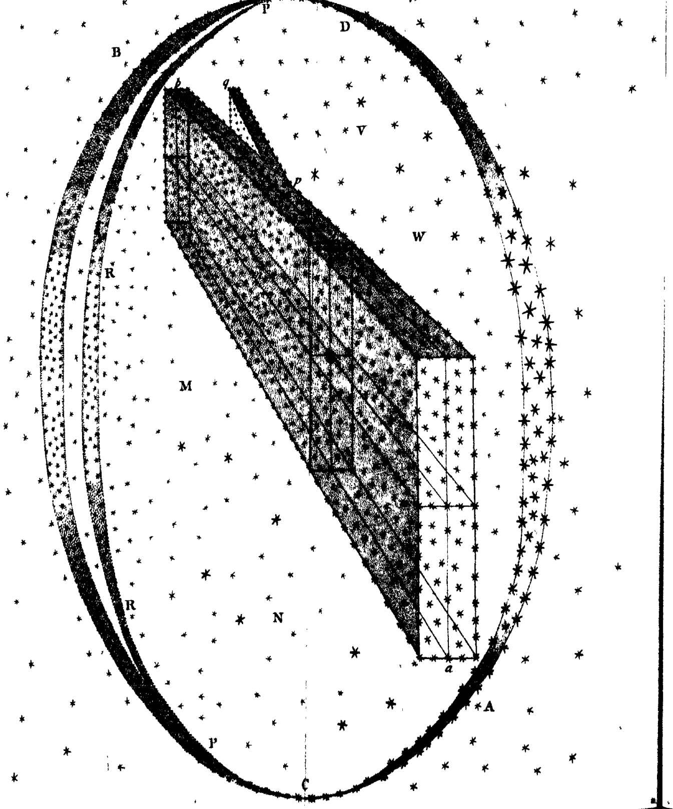


Fig. 17.

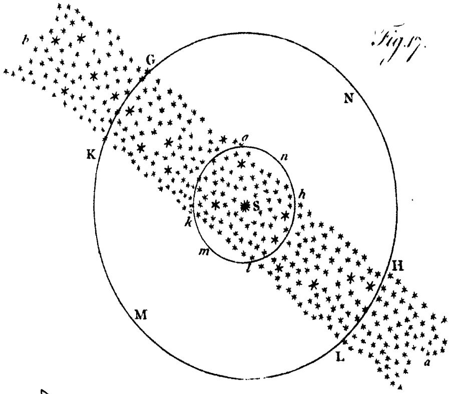
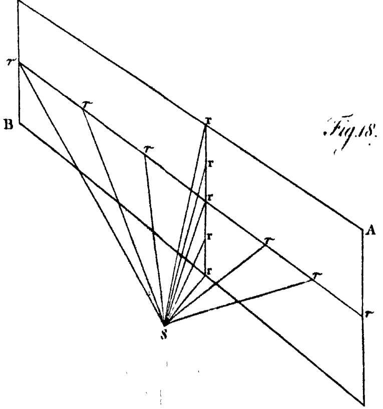


Fig. 18.



with ſmall, ſeeming attendants; narrow but much extended, lucid nebulæ or bright daſhes; ſome of the ſhape of a fan, reſembling an electric bruſh, iſſuing from a lucid point; others of the cometic ſhape, with a ſeeming nucleus in the center; or like cloudy ſtars, ſurrounded with a nebulous atmoſphere; a different ſort again contain a nebuloſity of the milky kind, like that wonderful, inexplicable phænomenon about θ Orionis; while others ſhine with a fainter, mottled kind of light, which denotes their being reſolvable into ſtars. See fig. 3. &c. But it would be too extenſive at preſent to enter more minutely into ſuch circumſtances, therefore I proceed with the ſubject of nebulous and ſidereal ſtrata.

It is very probable, that the great ſtratum, called the milky way, is that in which the ſun is placed, though perhaps not in the very center of its thickneſs. We gather this from the appearance of the Galaxy, which ſeems to encompaſs the whole heavens, as it certainly muſt do if the ſun is within the ſame. For, ſuppoſe a number of ſtars arranged between two parallel planes, indefinitely extended every way, but at a given conſiderable diſtance from each other; and, calling this a ſidereal ſtratum, an eye placed ſomewhere within it will ſee all the ſtars in the direction of the planes of the ſtratum projected into a great circle, which will appear lucid on account of the accumulation of the ſtars; while the reſt of the heavens, at the ſides, will only ſeem to be ſcattered over with conſtellations, more or leſs crowded, according to the diſtance of the planes or number of ſtars contained in the thickneſs or ſides of the ſtratum.

Thus, in fig. 16. (tab. XVIII.) an eye at S within the ſtratum *a b*, will ſee the ſtars in the direction of its length *a b*, or height *c d*, with all thoſe in the intermediate ſituations, projected into the lucid circle ACBD; while thoſe in the ſides mv, nw, will be ſeen ſcattered over the remaining part of the heavens at MVNW.

If the eye were placed ſomewhere without the ſtratum, at no very great diſtance, the appearance of the ſtars within it would aſſume the form of one of the leſs circles of the ſphere, which would be more or leſs contracted to the diſtance of the eye; and if this diſtance were exceedingly increaſed, the whole ſtratum might at laſt be drawn together into a lucid ſpot of any ſhape, according to the poſition, length, and height of the ſtratum.

Let us now ſuppoſe, that a branch, or ſmaller ſtratum, ſhould run out from the former, in a certain direction, and let it alſo be contained between two parallel planes extended indefinitely onwards, but ſo that the eye may be placed in the great ſtratum ſomewhere before the ſeparation, and not far from the place where the ſtrata are ſtill united. Then will this ſecond ſtratum not be projected into a bright circle like the former, but will be ſeen as a lucid branch proceeding from the firſt, and returning to it again at a certain diſtance leſs than a ſemi-circle.

Thus, in the ſame figure, the ſtars in the ſmall ſtratum pq will be projected into a bright arch at PRRP, which, after its ſeparation from the circle CBD, unites with it again at P.

What has been inſtanced in parallel planes may eaſily be applied to ſtrata irregularly bounded, and running in various directions; for their projections will of conſequence vary according to the quantities of the variations in the ſtrata and the diſtance of the eye from the ſame. And thus any kind of curvatures, as well as various different degrees of brightneſs, may be produced in the projections.

From appearances then, as I obſerved before, we may infer, that the ſun is moſt likely placed in one of the great ſtrata of the fixed ſtars, and very probably not far from the place where ſome ſmaller ſtratum branches out from it. Such a ſuppoſition will ſatisfactorily, and with great ſimplicity, account for all the phænomena of the milky way, which, according to this hypotheſis, is no other than the appearance of the projection of the ſtars contained in this ſtratum and its ſecondary branch. As a farther inducement to look on the Galaxy in this point of view, let it be conſidered, that we can no longer doubt of its whitiſh appearance ariſing from the mixed luſtre of the numberleſs ſtars that compoſe it. Now, ſhould we imagine it to be an irregular ring of ſtars, in the center nearly of which we muſt then ſuppoſe the ſun to be placed, it will appear not a little extraordinary, that the ſun, being a fixed ſtar like thoſe which compoſe this imagined ring, ſhould juſt be in the center of ſuch a multitude of celeſtial bodies, without any apparent reaſon for this ſingular diſtinction; whereas, on our ſuppoſition, every ſtar in this ſtratum, not very near the termination of its length or height, will be ſo placed as alſo to have its own Galaxy, with only ſuch variations in the form and luſtre of it, as may ariſe from the particular ſituation of each ſtar.

Various methods may be purſued to come to a full knowledge of the ſun's place in the ſidereal ſtratum, of which I ſhall only mention one as the moſt general and moſt proper for determining this important point, and which I have already begun to put in practice. I call it *Gaging the Heavens*, or the *Star-Gage*. It conſiſts in repeatedly taking the number of ſtars in ten fields of view of my reflector very near each other, and by adding their ſums, and cutting off one decimal on the right, a mean of the contents of the heavens, in all the parts which are thus gaged, is obtained. By way of example, I have joined a ſhort table, extracted from the gages contained in my journal, by which it appears, that the number of ſtars increaſes very faſt as we approach the Via Lactea.

N. P. D. 92 to 94°.

R. A.	Gage.
15 10	9.4
15 22	10.6
15 47	10.6
16 8	12.1
16 25	13.6
16 37	18.6

N. P. D. 78 to 80°.

R. A.	Gage.
11 16	3.1
12 31	3.4
12 44	4.6
12 49	3.9
13 5	3.8
14 30	3.6

Thus, in the parallel from 92 to 94 degrees north polar diſtance, and R. A. 15 h. 10′, the ſtar-gage runs up from 9.4 ſtars in the field to 18.6 in about an hour and a half; whereas in the parallel from 78° to 80° north polar diſtance, and R. A. 11, 12, 13, and 14 hours, it very ſeldom riſes above 4. We are, however, to remember, that with different inſtruments the account of the gages will be very different, eſpecially on our ſuppoſition of the ſituation of the ſun in a ſtratum of ſtars. For, let *a b*, fig. 17. be the ſtratum, and ſuppoſe the ſmall circle *g h l k* to repreſent the ſpace into which, by the light and power of a given teleſcope, we may penetrate; and let GHLK be the extent of another portion, which we are enabled to viſit by means of a larger aperture and power; it is evident, that the gages with the latter inſtrument will differ very much in their account of ſtars contained at MN, and at KG or LH; when with the former they will hardly be affected by the change from *m n* to *k g* or *l h*. And this accounts for what a celebrated author ſays concerning the effects of teleſcopes,

ſcopes, by which we muſt underſtand the beſt of thoſe that are in common uſe *.

It would not be ſafe to enter into an application of theſe, and ſuch other gages as I have already taken, till they are ſufficiently continued and carried all over the heavens. I ſhall, therefore, content myſelf with juſt mentioning that the ſituation of the ſun will be obtained, from conſidering in what manner the ſtar-gage agrees with the length of a ray revolving in ſeveral directions about an aſſumed point, and cut off by the bounds of the ſtratum. Thus, in fig. 18. let S be the place of an obſerver; S r r r, S *r r r*, lines in the planes r S r, *r* S *r*, drawn from S within the ſtratum to one of the boundaries, here repreſented by the plane AB. Then, ſince neither the ſituation of S, nor the form of the limiting ſurface AB, is given, we are to aſſume a point, and apply to it lines proportional to the ſeveral gages that have been obtained, and at ſuch angles from each other as they may point out; then will the termination of theſe lines delineate the boundary of the ſtratum, and conſequently manifeſt the ſituation of the ſun within the ſame. But to proceed.

If the ſun ſhould be placed in the great ſidereal ſtratum of the milky way, and, as we have ſurmiſed above, not far from the branching out of a ſecondary ſtratum, it will very naturally lead us to gueſs at the cauſe of the probable motion of the ſolar ſyſtem: for the very bright, great node of the Via Lactis, or union of the two ſtrata about Cepheus and Caſſiopeia, and the Scorpion and Sagittarius, points out a conflux of ſtars manifeſtly quite ſufficient to occaſion a tendency towards that node in any ſtar ſituated at no very great diſtance; and the ſecondary branch of the Galaxy not being much leſs than a ſemi-circle ſeems to indicate ſuch a ſituation of our ſolar ſyſtem in the great undivided ſtratum as the moſt probable.

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* On voit avec les téleſcopes des étoiles dans toutes les parties du ciel, à peu près comme dans la voie lactée, ou dans les nébuleuſes. On ne ſauroit douter qu'une partie de l'éclat et de la blancheur de la voie lactée, ne provienne de la lumière des petites étoiles qui s'y trouvent en effet par millions; cependant, avec les plus grands téleſcopes, on n'en diſtingue pas aſſés, et elles n'y ſont pas aſſés rapprochées les unes des autres pour qu'on puiſſe attribuer à celles qu'on diſtingue la blancheur de la voie lactée, ſi ſenſible à la vue ſimple. L'on ne ſauroit donc prononcer que les étoiles ſoient la ſeule cauſe de cette blancheur, quoique nous ne connoiſſions aucune maniére ſatisfaiſante de l'expliquer. Aſt. M. De la Lande, § 833.

What has been ſaid in a former paper on the ſubject of the ſolar motion ſeems alſo to ſupport this ſuppoſed ſituation of the ſun; for the apex there aſſigned lies nearly in the direction of a motion of the ſun towards the node of the ſtrata. Beſides, the joining ſtratum making a pretty large angle at the junction with the primary one, it may eaſily be admitted, that the motion of a ſtar in the great ſtratum, eſpecially if ſituated conſiderably towards the ſide fartheſt from the ſmall ſtratum, will be turned ſufficiently out of the ſtraight direction of the great ſtratum towards the ſecondary one. But I find myſelf inſenſibly led to ſay more on this ſubject than I am as yet authoriſed to do; I will, therefore, return to thoſe obſervations which have ſuggeſted the idea of celeſtial ſtrata.

In my late obſervations on nebulæ I ſoon found, that I generally detected them in certain directions rather than in others; that the ſpaces preceding them were generally quite deprived of their ſtars, ſo as often to afford many fields without a ſingle ſtar in it; that the nebulæ generally appeared ſome time after among ſtars of a certain conſiderable ſize, and but ſeldom among very ſmall ſtars; that when I came to one nebula, I generally found ſeveral more in the neighbourhood; that afterwards a conſiderable time paſſed before I came to another parcel; and theſe events being often repeated in different altitudes of my inſtrument, and ſome of them at a conſiderable diſtance from each other, it occurred to me, that the intermediate ſpaces between the ſweeps might alſo contain nebulæ; and finding this to hold good more than once, I ventured to give notice to my aſſiſtant at the clock, "to prepare, ſince I expected in a few minutes to come at a ſtratum of the nebulæ, finding myſelf already" (as I then figuratively expreſſed it) "on nebulous ground." In this I ſucceeded immediately; ſo that I now can venture to point out ſeveral not far diſtant places, where I ſhall ſoon carry my teleſcope, in expectation of meeting with many nebulæ. But how far theſe circumſtances of vacant places preceding and following the nebulous ſtrata, and their being as it were contained in a bed of ſtars, ſparingly ſcattered between them, may hold good in more diſtant portions of the heavens, and which I have not yet been able to viſit in any regular manner, I ought by no means to hazard a conjecture. The ſubject is new, and we muſt attend to obſervations, and be guided by them, before we form general opinions.

Before I conclude, I may, however, venture to add a few particulars about the direction of ſome of the capital ſtrata or their branches. The well known nebula of Cancer, viſible to the naked eye, is probably one belonging to a certain ſtratum, in which I ſuppoſe it to be ſo placed as to lie neareſt to us. This ſtratum I ſhall call that of Cancer. It runs from ε Cancri towards the ſouth over the 67 nebula of the *Connoiſſance des Temps*, which is a very beautiful and pretty much compreſſed cluſter of ſtars, eaſily to be ſeen by any good teleſcope, and in which I have obſerved above 200 ſtars at once in the field of view of my great reflector, with a power of 157. This cluſter appearing ſo plainly with any good, common teleſcope, and being ſo near to the one which may be ſeen by the naked eye, denotes it to be probably the next in diſtance to that within the quartile formed by γ, δ, η, θ; from the 67th nebula the ſtratum of Cancer proceeds towards the head of Hydra; but I have not yet had time to trace it farther than the equator.

Another ſtratum, which perhaps approaches nearer to the ſolar ſyſtem than any of the reſt, and whoſe ſituation is nearly at rectangles to the great ſidereal ſtratum in which the ſun is placed, is that of Coma Berenices, as I ſhall call it. I ſuppoſe the Coma itſelf to be one of the cluſters in it, and that, on account of its nearneſs, it appears to be ſo ſcattered. It has many capital nebulæ very near it; and in all probability this ſtratum runs on a very conſiderable way. It may, perhaps, even make the circuit of the heavens, though very likely not in one of the great circles of the ſphere: for, unleſs it ſhould chance to interſect the great ſidereal ſtratum of the milky way before-mentioned, in the very place in which the ſun is ſtationed, ſuch an appearance could hardly be produced. However, if the ſtratum of Coma Berenices ſhould extend ſo far as (by taking in the aſſiſtance of M. MESSIER's and M. MECHAIN's excellent obſervations of ſcattered nebulæ, and ſome detached former obſervations of my own) I apprehend it may, the direction of it towards the north lies probably, with ſome windings, through the great Bear onwards to Caſſiopeia; thence through the girdle of Andromeda and the northern Fiſh, proceeding towards Cetus; while towards the ſouth it paſſes through the Virgin, probably on to the tail of Hydra and the head of Centaurus. But, notwithſtanding I have already fully aſcertained the exiſtence and direction of this ſtratum for more than 30 degrees of a great circle, and found it almoſt every where equally rich in fine nebulæ, it ſtill might be dangerous to proceed in more extenſive conjectures, that have as yet no more than a precarious foundation. I ſhall therefore wait till the obſervations in which I am at preſent engaged ſhall furniſh me with proper materials for the diſquiſition of ſo new a ſubject. And though my ſingle endeavours ſhould not ſucceed in a work that ſeems to require the joint effort of every aſtronomer, yet ſo much we may venture to hope, that, by applying ourſelves with all our powers to the improvement of teleſcopes, which I look upon as yet in their infant ſtate, and turning them with aſſiduity to the ſtudy of the heavens, we ſhall in time obtain ſome faint knowledge of, and perhaps be able partly to delineate, *the Interior Conſtruction of the Univerſe.*

Datchet near Windſor,
April, 1784.

WILLIAM HERSCHEL.

XXXIV. *An Account of a new Species of the* Bark-Tree, *found in the Iſland of St. Lucia. By Mr.* George Davidſon; *communicated by* Donald Monro, *M. D. Phyſician to the Army, F. R. S.*

Read June 24, 1784.

DR. DONALD MONRO.

SIR,

HAVING received from my correſpondent Mr. DAVIDSON, ſurgeon, in the iſland of St. Lucia, ſome Bark, the product of that iſland, which is ſaid to poſſeſs the virtues of the Jeſuit's Bark, and in a much ſmaller doſe, I ſhall eſteem it a favour if you will lay before the Royal Society the ſpecimen which I have ſent to you with this letter, together with Mr. DAVIDSON's account of it, if you think they merit that honour.

I have examined the dried ſpecimens very carefully. They are not ſo well preſerved as I could wiſh; but I have ſince ſeen much finer in the poſſeſſion of Sir JOSEPH BANKS, who has done me the honour to favour me with the following character, as moſt diſtinctive of it from the other ſpecies of Cinchona already deſcribed, which he gave me an opportunity of examining.

It is undoubtedly a Cinchona, but not the Cinchona officinalis of LINNÆUS; for it differs from it eſſentially in its bark in ſeveral particulars. It has an emetic quality not common to the true bark, breaks more woody and ſplintery, and is far more nauſeous to the taſte. Its decoction is of a dull Burgundy colour; and its extract reſembles more the bitter of Gentian than that of the Quinquina. I have procured four ounces of it from half a pound of the Bark boiled in water, and herewith ſend to you a ſmall ſpecimen.

The drawings, which accompany this letter, are exact copies of the ſpecimens which I received; I therefore hope they will not be thought unworthy the acceptance of the Royal Society.

I have the honour to be, &c.

Henrietta-ſtreet, Nov. 6, 1783. G. WILSON.

Botanic character of the Bark-Tree of St. Lucia.

" Cinchona floribus paniculatis, glabris; laciniis linearibus,
" tubo longioribus; ſtaminibus exſertis; foliis ellipticis, glabris."

Extract of a Letter from Mr. GEORGE DAVIDSON, *dated* St. Lucia, July 15, 1783.

IT is now about four years ſince Mr. ALEXANDER ANDERSON diſcovered in the woods, near the Grand Cul de Sac, ſome trees reſembling, in the botanical characters, the true Quinquina of LINNÆUS. He brought the bark, flowers, and ſeeds, to Dr. YOUNG of the General Hoſpital, and trial was made of it there; but not being ſufficiently dried, its ſtrong emetic and purgative qualities prevented its exhibition.

The publication of Dr. SAUNDERS, which I received about two months ago, mentioning the introduction of a ſpecies of bark of a redder colour, and poſſeſſing greater powers than the bark formerly in uſe, induced us here to try the bark of this country. Dr. YOUNG had by him ſome that was collected in General GRANT's time: on account of the length of time it had been kept, and its being ſufficiently dried, he has met with all the ſucceſs he could wiſh.

It is manifeſtly more aſtringent than the bark, and the bitter is likewiſe more durable on the palate.

Hitherto I have generally uſed the cold infuſion, either in lime or ſimple water, in the proportion of one ounce to three pints of the water. I have likewiſe given it in ſubſtance from twenty to thirty grains; but never exceeded the laſt quantity, for I never found the ſtomach able to retain more than twenty grains.

Joined with the Canella alba, it forms in ſpirits an agreeable and elegant tincture. I have made a tincture from the ſeeds, which are infinitely ſtronger in taſte than the bark itſelf.

(Signed) GEO. DAVIDSON.

Mr. George Davidſon's *account of the* Bark-Tree *of the iſland of* St. Lucia.

THE Bark-Tree of this iſland is nearly about the ſize of the cherry-tree, ſeldom thicker than the thigh, and tolerably ſtraight; the wood is light and porous, without any of the bitterneſs and aſtringency of the bark itſelf.

It delights in a ſhady ſituation, the north-weſt aſpect of hills, under larger trees; and is generally to be found about the middle of an hill, near ſome running water.

The leaves are large, oblong, oppoſite, and plain, preſerving (as well as the flowers and ſeeds) the bitter taſte of the bark.

In the beginning of the rainy ſeaſon (June), the tree puts forth its flowers in ſmall tufts; at firſt they are white, but afterwards turn purpliſh. The ſtamina are five in number, with a ſingle ſtyle. The germen is oblong, bilocular, and furrowed on each ſide. The ſeeds are many, and of the winged kind. The corolla is monopetalous, with its mouth divided into five long ſegments.

The ſoil in general where it grows is a ſtiff red clay. The bark itſelf is of a lighter red than that ſent out here to the hoſpital under the name of *red bark.* It inclines more to the colour of cinnamon. The bitterneſs and aſtringency appear to be greater than in either of the other barks.

I apprehend, the proper ſeaſon for obtaining it is about the month of March, before the flowers come out: after-experience will beſt determine this.

Infuſed in cold water, in which form, or in lime-water, I generally uſe it, it forms a very red tincture, poſſeſſing the bitterneſs and aſtringency of the bark very ſtrongly. A few drops of the Tinctura florum martialium give it a very black colour, and occaſion a copious depoſition of a black ſediment. It does the ſame with the ſpirituous tincture.

With ſpirits it forms a beautiful red tincture.

Explanation of the references tab. XIX.

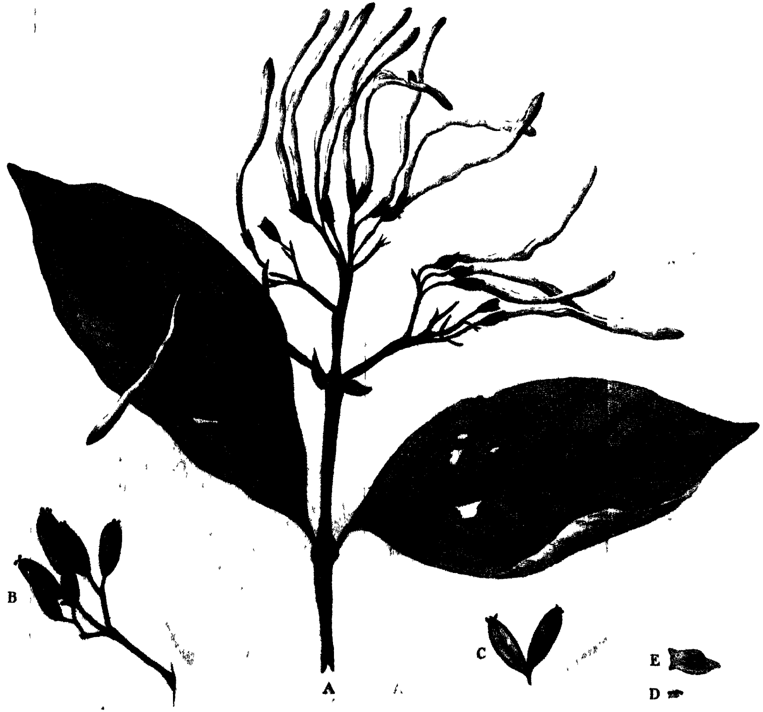
A. A branch of the Cinchona of St Lucia, with the flowers not yet opened.

B. The entire ſeed-veſſels.

C. A ſeed-veſſel ſplit.

D. One of the ſeeds, of its natural ſize.

E. The ſame magnified.


A
B
C
D
E

XXXV. *An Account of an Obſervation of the Meteor of* Auguſt 18, 1783, *made on* Hewit Common *near* York. *In a Letter from* Nathaniel Pigott, *Eſq. F. R. S. to the Reverend* Nevil Maſkelyne, *D. D. F. R. S. and Aſtronomer Royal.*

Read June 24, 1784.

REVEREND SIR, York, Oct. 18, 1783.

ON the 19th of laſt Auguſt I communicated to you an account of the remarkably fine meteor, which I had ſeen under circumſtances peculiarly favourable the preceding night. I was then preparing myſelf for a journey into the Eaſt Riding; and, on that account, obliged to poſtpone the verifications, mentioned hereafter, till my return.

On the 18th of Auguſt, about ten o'clock P.M. after a hot day, the weather a little hazy, but not ſo as to obliterate the ſtars, and no wind, being on horſeback, in company with two other gentlemen, on Hewit Common, about three miles from York, my attention was attracted towards the W.N.W. by ſeveral faint flaſhes of lightning, ſuch as are often ſeen near the horizon, or which may be ſtill better compared to flaſhes of an aurora borealis. Soon after which I perceived ſome luminous matter in motion, and collecting together from ſeveral directions, fig. 1. (tab. XX.) which immediately taking fire preſented itſelf under the form of a ball, of ſo vivid a brightneſs, that the whole horizon was illuminated, ſo that the ſmalleſt object might

have

have been ſeen on the ground. This ball, when formed, began to move, with an eaſy ſliding motion, from W.N.W. towards the S.S.E. It ſuggeſted the idea of a highly brilliant comet, emitting a train or tail, but of a different colour from the ball itſelf, this laſt being of a moſt brilliant bluiſh white, and the tail of a duſky red; the length of which appeared to extend over fifteen or more degrees of the heavens, fig. 2. The apparent diameter of the nucleus ſeemed one-third or one-fourth of the full moon's diameter. The greateſt difficulty in this eſtimation hence ariſes, that I cannot, notwithſtanding all my endeavours, repreſent in my mind the moon otherwiſe than as a plane or diſk; nor the meteor, than as a ſpherical body. The altitude of it, when it formed in the W.N.W. was about 30°; and about 19° or 20° above the horizon, when it became extinct in the S.S.E. a few ſparks of the tail, neareſt the nucleus, ſcattering themſelves much in the ſame manner as thoſe of a ſky-rocket when burnt out, fig. 3.

It has been ſaid, that the ball divided itſelf into three or four parts before its extinction. To me it appeared to vaniſh or gently die away: what confirms me in the opinion, that it did not divide, is, that the three or four ſcattering parts above-mentioned were not of the bright colour of the ball itſelf, but of the duſky red which the tail invariably ſhewed. The interval of time from the meteor's formation to its extinction was nearly twenty ſeconds, perhaps two or three ſeconds leſs. The long habit I have of counting ſeconds in aſtronomical obſervations induces me to think this quantity may be relied on; and this I mention, becauſe ſome have eſtimated it more, ſome leſs. Nine or ten minutes after its diſſipation, I heard a noiſe, much reſembling the report of a cannon at a very great diſtance; but I would not wiſh to have it underſtood, that I ſpeak to this laſt interval with the ſame certainty as to the other; if, however, it be exact, and ſuppoſing ſound to move 1106 feet in one ſecond of time, and the ſame in the upper regions of the atmoſphere as here below, which, however, may be very different, its diſtance from me, at its extinction, muſt have been about 120 miles, and its perpendicular altitude above the earth's ſurface about 40 miles.

I have added a ſcheme and a ſmall ſketch, preſuming by that means to convey a clearer idea of what I ſaw. The altitudes, azimuths, &c. are not merely from eſtimation. After my return from the Eaſt Riding, I went to the very ſpot, where I had ſeen the meteor on the 18th of Auguſt. The road, as in the ſcheme, being exactly ſtraight from my ſtation, both *towards* and *from* York, no miſtake can ariſe in that reſpect. With all the circumſtances clearly and forcibly impreſſed on my mind, I watched till ſome remarkable ſpot in the ſky preſented itſelf at the ſame place in which I had ſeen the meteor itſelf form, croſs the road, vaniſh, &c.: then, with a theodolite, I took the ſeveral bearings, which may be the more relied on, as I repeated the operations three different times, on different ſpots, which agree ſurpriſingly well for meaſures where no minute exactneſs can be expected. I have marked minutes in the ſcheme, becauſe the reſults gave them, without any pretenſion to ſuch nicety.

I am, &c.

NATH. PIGOTT.

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XXXVI. *Obſervations of the Comet of* 1783. *In a Letter from* Edward Pigott, *Eſq. to the Rev.* Nevil Maſkelyne, *D. D. F. R. S. and Aſtronomer Royal.*

Read June 24, 1784.

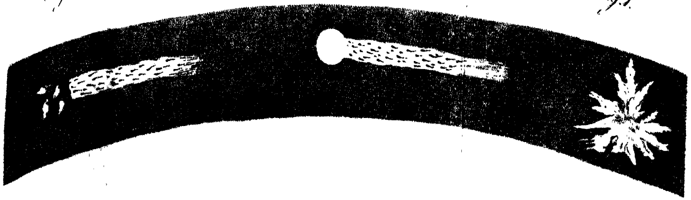
REVEREND SIR, York, Dec 11, 1783.

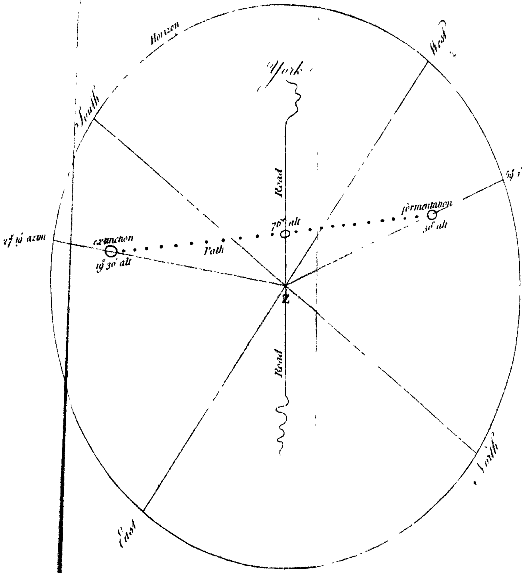
HAVING compleated my obſervations of the comet I diſcovered on the 19th of November laſt, I take the liberty of deſiring you to preſent them to the Royal Society. The faintneſs of the comet's light, and the unfavourable ſky you have had in the ſouth, induce me to believe, that few obſervations of it have been made beſides the following.

Dates.	Apparent time.	R. A.	North declination.	Greateſt error of each R. A.	Longitude.	Latitude.
1783	h. ′	° ′ ″	° ′ ″	′ ″	S. ° ′	° ′
Novem. 19	11 4	41 1 30	3 11 ½	3 00	1 9 37	12 4½ S.
20	10 55−	40 0 3	4 32 ¾	0 22	1 9 2¾	10 31
22	6 52+	38 21 10	6 50	0 30	1 8 11	7 50
24	10 24−	36 29 28	9 36 ¼	0 15	1 7 19	4 52¼
26	10 9−	34 49 20	12 3 ¼	0 15	1 6 33⅔	2 6
Decem. 3	15 54	29 21 50	20 15 ¼	0 40	1 4 24	7 42½ N

The R. A's of November 20th, 24th, and 26th, were deduced from obſervations made at the tranſit inſtrument: the others, except the firſt, were determined with an excellent 2½ feet night-glaſs, made by Dollond, magnifying 20 times, having croſs wires at right angles in its focus, which were

Fig. 3 Fig. 2 Fig. 1





viſible without being illuminated. With this inſtrument the comet, by the common method, was compared to ſtars in the field of the teleſcope, and within four minutes on the ſame parallel. The places of thoſe ſtars were afterwards ſettled with the meridian inſtruments. As ſometimes ſeveral ſtars were obſerved, I eaſily found to what degree of certainty thoſe obſervations might be depended on, which I have marked with the above reſults. The declinations, I think, cannot err two minutes, being compared to ſtars within four minutes on the ſame parallel. The three of November 20th, 24th, and 26th, were taken with the tranſit inſtrument by comparing the comet to the neareſt ſtars. I was much chagrined in not being able to ſee the comet in our equatorial when the wires were illuminated.

The comet had exactly the appearance of a nebula: its light was ſo faint that it could not be ſeen in a good opera glaſs. In the night-teleſcope the nucleus was ſcarcely viſible, and the diameter of the ſurrounding coma was about three minutes of a degree. Between the 19th and 26th of November, I thought it had rather diminiſhed in brightneſs. December the 1ſt and 3d it was very difficult to be ſeen, occaſioned perhaps by its little elevation above the horizon. Between December the 3d and 10th, the comet was entirely effaced by the increaſed light of the Moon. On the 10th, the moon being in the horizon did not obliterate ſtars of the eighth or ninth magnitude; but I could not find the comet. The following obſervations were made by my friend Mr. JOHN GOODRICKE.

Dates.	Apparent time.	R. A.	North declination.	Longitude.	Latitude.
1783	h. ′	° ′ ″	° ′	S. ° ′	° ′
Novem. 24	8 16	36 32 57	9 30¾	1 7 21	1 42½S
28	6 8½	33 20 0	14 16½	1 5 55½	0 52½N

I am, &c.

EDW. PIGOTT.

P. S. This morning I received a letter from M. DE MECHAIN, in which he informs me, that he diſcovered the comet on the 26th of November ſeven days after my firſt obſervation. He has made ſeveral obſervations on it.

XXXVII. *Experiments on mixing Gold with Tin. In a Letter from Mr.* Staneſby Alchorne, *of his Majeſty's Mint, to* Peter Woulfe, *Eſq. F. R. S.*

Read June 24, 1784.

Tower of London, March 25, 1784.

DEAR SIR,

YOU know it is a generally received opinion among metallurgiſts, that tin has a property of deſtroying the ductility of gold, on being melted with it, even in very ſmall quantities. Our late ingenious countryman Dr. LEWIS, in his Philoſophical Commerce of Arts, p. 85. has well expreſſed the ſenſe of moſt writers on this ſubject, in the following words: "The moſt minute proportion of tin and lead," ſays he, "and even the vapours which riſe from them in the fire, "though not ſufficient to add to the gold any weight ſenſible "in the tendereſt balance, make it ſo brittle that it flies in "pieces under the hammer."

Divers circumſtances, nevertheleſs, long ſince induced me to diſbelieve the fact; but theſe, having chiefly ariſen from ſmall experiments, did not ſeem to warrant any general concluſion. A late public occaſion, however, which led me to various trials of mixing theſe metals together, in different proportions, and in ſufficiently large quantities, has put the matter out of diſpute; and ſhewn me, that tin, in ſmall quantity at leaſt, may be added to gold, either pure or alloyed, without producing any other effect than what might eaſily be conceived,

ceived, *à priori*, from the different texture of the two metals. In confirmation of which, I beg leave to lay ſome of the experiments before you.

EXPERIMENT I.

Sixty Troy grains of pure tin were ſtirred into twelve ounces of refined gold, in fuſion; and the mixture was then caſt into a mould of ſand, producing a flat bar, one inch wide, and one-eighth of an inch thick. The bar appeared ſound and good, ſuffered flatting under the hammer, drawing ſeveral times between a large pair of ſteel rollers, and cutting into circular pieces, of near an inch diameter, which bore ſtamping in the money-preſs, by the uſual ſtroke, without ſhewing the leaſt ſign of brittleneſs; or rather with much the ſame ductility as pure gold.

EXPERIMENT II.

Ninety grains of like tin were added to twelve ounces of fine gold, ſtirred, and caſt as above. The bar produced was ſcarcely diſtinguiſhable from the former, and bore all the operations, as before-mentioned, quite as well.

EXPERIMENT III.

One hundred and twenty grains of fine tin were mixed with twelve ounces of fine gold, and being caſt like the foregoing, produced a bar rather paler and harder than the preceding, but which ſuffered the like operations very well; except that, on drawing between rollers, the outer edges were diſpoſed to crack a little.

EXPERIMENT IV.

One hundred and forty grains, or half an ounce, of the like grained tin, were mixed, as before, with twelve ounces of fine gold; and the bar reſulting from this mixture was completely ſound and good; evidently paler and harder, however, than any of the foregoing, and cracking rather more than the laſt on paſſing between the rollers; but bearing every other operation, even ſtamping under the preſs, by the common force, without any apparent injury.

EXPERIMENT V.

One ounce of tin was next ſtirred into twelve ounces of the like refined gold, and then caſt as before; but the bar produced, though ſeemingly ſolid and good, was bad coloured, brittle in texture, and, on the firſt paſſing between the rollers, ſplit into ſeveral pieces, ſo that no farther trials were made with it.

EXPERIMENT VI.

To inquire how far the fumes of tin, brought into contact with the gold, would do more than mixing the metal in ſubſtance, a ſmall crucible, filled with twelve ounces of ſtandard gold, $\frac{11}{12}$ fine, was placed in a larger crucible, having one ounce of melted tin in it, and kept there in fuſion, the whole being covered by another large inverted crucible, for about half an hour. In this time a full quarter part of the tin was calcined; but the gold remained unaltered, and equally capable of being manufactured as another portion of the ſame gold melted in the common manner.

It may well be aſked, whether the tin, or part of it, in every trial, might not be deſtroyed, and thus render the concluſions fallacious? But as, in any of theſe experiments, not more than ſix or eight grains of the original weight were miſſing after the caſting, and as even fine gold can ſcarcely be melted without ſome loſs in the operation, ſo we may reaſonably ſuppoſe, that our ſmall loſſes, in the foregoing trials, do not deſerve conſideration.

The above experiments then ſeem to ſhew, that tin is not ſo miſchievous to gold as hath been generally repreſented. But it would be unfair to infer, that the original author of this doctrine (from whom ſo many have implicitly tranſcribed) had no foundation for the aſſertion. Gold and Tin, indeed, are ſubſtances pretty well known; but it is eaſy to imagine, that coins or trinkets may have been uſed for one, and impure tin, or pewter, perhaps, for the other; and it is difficult to gueſs what might be the reſult of ſuch uncertain combinations. To inquire farther, therefore, the experiments were continued as follows.

EXPERIMENT VII.

To determine whether the two metals might be more intimately combined, and the maſs rendered brittle, by additional heat; the mixture of gold and tin, produced in the firſt of theſe experiments, was re-melted in a ſtronger fire than before, and thus kept in fuſion full half an hour. By this operation ſix grains only were loſt in the weight; and the bar obtained was no leſs manufacturable than at firſt.

EXPERIMENTS VIII. AND IX.

The mixtures of gold and tin, from the ſecond and fourth experiments, were re-melted ſeparately, and one ounce of copper added to each. Being both well ſtirred, they were caſt as uſual; and the bars, though ſenſibly harder, bore all the operations of manufacturing as before. The laſt bar cracked a little at the edges, on drawing through the rollers, as it had done without the copper, but not materially, and bore cutting rather better than in its former ſtate.

EXPERIMENTS X. AND XI.

A quarter of an ounce of the laſt mixture (being tin half an ounce, and copper one ounce, with gold twelve ounces), and as much of the bar from experiment the third (being tin one hundred and twenty grains with gold twelve ounces), were each melted by a Jeweller, in the moſt ordinary manner, with a common ſea-coal fire, into ſmall buttons, without any loſs of weight. Theſe buttons were forged by him into ſmall bars, nealing them often by the flame of a lamp, and afterwards drawn each about twenty times through the apertures of a ſteel plate, into fine wire, with as much eaſe as coarſe gold commonly paſſes the like operation.

EXPERIMENT XII.

To enquire whether the adding of tin to gold, already alloyed, would cauſe any difference, ſixty grains of tin were ſtirred into twelve ounces of ſtandard gold, $\frac{11}{12}$ fine; and the reſult paſſed every operation before deſcribed, without ſhewing the leaſt alteration from the tin.

For greater certainty, ſeveral other trials were made, of different mixtures of copper, tin, and ſilver, with gold, even ſo low as two ounces and a half of copper, with half an ounce of tin, to twelve ounces of gold. But theſe are not worth particularizing; for they all bore hammering, and flatting by rollers, to the thinneſs of ſtiff paper, and afterwards working into watch-caſes, cane-heads, &c. with great eaſe. They all, indeed, grew more hard and harſh, in proportion to the quantity of alloy; but not one of them had the appearance of what all workmen well know by the name of brittle gold. Whence it ſhould ſeem, that neither tin in ſubſtance, or the fumes of it, tend much to render gold unmanufacturable.

Whenever, therefore, brittleneſs has followed the adding ſmall quantities of tin to fine gold, it muſt be ſuppoſed to have ariſen from ſome unfriendly mixture in the tin, probably from Arſenic; for other experiments have ſhewn me, that twelve grains of regulus of arſenic, injected into as many ounces of fine gold, will render it totally unmalleable.

From the foregoing experiments, I preſume, we may fairly conclude, that though tin, like other inferior metals, will contaminate gold, in proportion to the quantity mixed with it, yet there does not appear any thing in it ſpecifically inimical to this precious metal. And this being contrary to the doctrine of moſt chemical writers, I ſubmit to your better judgement, whether it may not be uſeful to publiſh theſe experiments, by laying them before the Royal Society.

I am, &c.

S. ALCHORNE.

XXXVIII. *Sur un moyen de donner la Direction aux Machines Aëroſtatiques. Par M. Le Comte* De Galvez. *Communicated by Sir* Joſeph Banks, *Bart. P. R. S.*

Read July 1, 1784.

NOUS ſouſſignés certifions, que M. le Comte DE GALVEZ nous ayant communiqué ſes idées ſur le moyen de pouvoir donner la direction aux machines aëroſtatiques, pour faire route à volonté et par un rumb certain et aſſuré dans l'air, principalement fondé ſur différentes obſervations qu'il avoit faites ſur l'uſage que les oiſeaux font de leurs aîles quand ils volent, et ſur celui que font les poiſſons de leurs nageoires et de leur queue quand ils nagent :

Nous nous ſommes tranſportés, l'apres-midi du prémier Mars de cette année 1784, au canal de Manzanarês, où on avoit preparée une chaloupe de vingt-cinq pieds de long ſur quatre et demi de large, avec une machine qu'il avoit inventée pour démontrer ſes idées. Cette machine *, qui conſiſtoit en un chevalet qui alloit de poupe à proue à la hauteur de cinq pieds, étoit croiſée en rectangles par trois vergues de bois élaſtique, de dix-huits pieds de long chacune, avec une aîle à chaque bout, compoſée de baguettes de baleine, couvertes d'un morceau de taffetas de cinq pieds de long, et trois de large, laquelle étoit jointe par un de ſes quatre côtés à la vergue, de façon que l'aîle reſtoit horiſontale. Le mouvement ſe communiquoit à chaque vergue, et par conſéquent à ſes deux aîles, par un ſeul homme, qui tirant avec viteſſe des cordes attachées aux bouts de chaque vergue, les agitoit verticalement, d'où reſultoit que

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* See tab. XXI. fig. 1.

quand elles ſe plioient, les aîles prenoient à leurs extrémités une inclination de quarante-cinq degrés de l'horiſon. Ce mouvement et celui de la réaction produiſoient dans la chaloupe, où il y avoit ſix hommes, une impulſion qui la faiſoit marcher contre le courant du canal et le peu d'air qu'il faiſoit, cent cinquante pieds par minute, outre ſoixante pieds qu'elle parcouroit avant de s'arrêter depuis l'inſtant qu'on ceſſoit de mouvoir les aîles: elle parcouroit deux cents quarante-trois pieds par minute, allant avec le courant et l'air, par le même mouvement continu des aîles.

Nous fûmes tous très-etonnés de l'effet que produiſit cette expérience; car, quoique le deſir qu'avoit l'inventeur de mettre ſes idées en pratique au plutôt, fut cauſe qu'il ſe ſervit d'une chaloupe lourde et mal conſtruite, avec laquelle les aîles n'avoient point de proportion; nous ſommes perſuadés que la ſituation des aîles et leur mouvement vertical, qui formoient lors qu'on les battoient un plan incliné, imitant en cela les oiſeaux et les poiſſons, fourniſſent un principe ſûr et certain pour donner une direction par quelque rumb que ſe ſoit, à toute eſpèce de corps qui nagent dans un fluide, et par conſéquent très-applicable aux nouvelles machines aëroſtatiques.

Cette invention nous paroît digne de l'approbation et de l'eloge des phyſiciens qui, ſans doute, employeront leurs efforts pour lui donner toute la perfection dont elle eſt ſuſceptible dans l'exécution de ſon mécaniſme.

Et pour conſtater que la dite expérience a été faite de la manière qu'on vient d'expoſer, nous avons ſigné la préſente certification, ainſi qu'un deſſein de la dite machine, à Madrid le deux Mars, mil ſept cent quatre-vingt-quatre. D. JOSEF DE VIEXA, D. AGUSTIN BETANCOURT Y MOLINA, D. RICARDO WORSLEY, RAIM DE S. LAURENT, CASIMIRE ORTEGA.

XXXIX. *An extraordinary Caſe of a Dropſy of the Ovarium, with ſome Remarks. By Mr.* Philip Meadows Martineau, *Surgeon to the* Norfolk *and* Norwich *Hoſpital; communicated by* John Hunter, *Eſq. F. R. S.*

Read July 1, 1784.

SARAH KIPPUS, a pauper in the city of Norwich, was, for many years, a patient of my father's, and, at his deceaſe, was under the care of Mr. SCOTT, as city ſurgeon, who obliged me many times by taking me to the poor woman, from whom I received the account of the early part of her diſeaſe.

Her complaints came on firſt after a miſcarriage at the age of 27. She had never been pregnant before; and her diſcharges at that time were ſo great as to bring her into a very weak condition. She ſoon perceived ſome uneaſineſs, attended with a ſwelling, on one ſide, which, after a few months, became too large to diſtinguiſh whether it was greater on one ſide or the other. As the ſwelling was found to ariſe from water, it was drawn off, which was in the year 1757. She was never afterwards pregnant; but the catamenia continued regularly till the uſual period of their ceſſation. When I firſt ſaw her, which was in the year 1780, ſhe had been many times tapped, and ſhe was then full of water. Her appearance was truly deplorable, not to ſay ſhocking. She was rather a low woman, and her body ſo large as almoſt wholly to obſcure her face, as well as every other part of her: with all ſhe was tolerably chearful, and ſeldom regarded the operation. I ſaw her juſt before we took away 106 pints of water, and I begged leave to take a meaſure of her. She was ſixty-ſeven inches and a half in circumference, and from the cartilago enſiformis to the os pubis thirty-four inches. Her legs were now greatly ſwelled; but this, and every other ſymptom of which ſhe complained, evidently aroſe from the quantity and weight of water. She neither ate nor drank much, and made but a ſmall quantity of urine.

The operation of drawing off the water was generally performed on a Sunday, as the moſt convenient day for her neighbours to aſſiſt her, and before the latter end of the week ſhe was able to walk very well. She was firſt tapped in the year 1757, and died in Auguſt 1783. Thus ſhe lived full twenty-five years with ſome intervals of eaſe, having eighty times undergone the operation, and in all had taken from her 6631 pints of water, or upwards of thirteen hogſheads.

I will ſubjoin the account of the dates, and the quantity drawn off at each time, as given me by Mr. SCOTT, obſerving that till 1769 no exact memorandum was kept, except of the number of times, although the quantity of water drawn off was always meaſured. By my father ſhe was tapped twenty-ſix times, averaged at 70 pints each time: by Mr. DONNE once, 73 pints, which makes 1683 pints from ſome part of the year 1757 to 1769. By Mr. SCOTT as follows;

1769.	Pints.
Mar. 16.	70
July 17.	72
Nov. 20.	78
Dec. 31.	70
	290
1770.	
April 15.	70
Aug. 11.	73
Dec. 4.	76
	219
1771.	
Mar. 22.	74
July 14.	78
Nov. 3.	79
	231
1772.	
Feb. 22.	79
June 6.	73
Sept. 12.	74
Dec. 12.	82
	308
1773.	
March 7.	78
May 29.	71
Aug. 29.	79
Dec. 5.	81
	309

1774.	Pints.
Mar. 13.	77
June 26.	89
Oct. 23.	92
	258
1775.	
Jan. 24.	94
May 28.	91
Sept. 13.	72
Dec. 16.	80
	337
1776.	
April 9.	84
July 28.	82
Nov. 27.	85
	251
1777.	
Mar. 16.	89
July 27.	90
Nov. 9.	98
	277
1778.	
March 8.	96
July 5.	99
Nov. 5.	105
	300

1779.	Pints.
Feb. 28.	106
June 13.	108
Aug. 17.	92
Oct. 24.	99
Dec. 10.	90
	495
1780.	
Feb. 6.	73
Apr. 23.	102
July 24.	106
Sept. 10.	95
Nov. 12.	98
	474
1781.	
Jan. 1.	100
Mar. 11.	94
June 25.	100
Oct. 14.	100
	394
1782.	
Jan. 13.	99
Mar. 18.	64
June 2.	74
Aug. 25.	98
Nov. 17.	90
	425

1783.	Pints.
Feb. 14.	104
May 11.	100
July 20.	98
Aug. 11 on opening	78
	380

Total 6631 pints.

In looking over this account it appears, that 108 pints was the greateſt quantity ever taken away at any one time; that ſhe was never tapped more than five times in any one year; and the largeſt quantity in a year was 495 pints. The moſt collected in the ſhorteſt ſpace of time was 95 pints in ſeven weeks, from July 24th to September 10th in 1780, which is very nearly two pints a day. It appears alſo, that in the laſt 14 years of her life, when a regular account was kept, ſhe increaſed faſter in the winter than in the ſummer months. If the ſix ſummer months from April to September incluſive are reckoned, ſhe loſt in the 14 years in 23 operations 1972 pints, and in the winter months from October to March incluſive, by 30 tappings, 2596 pints; and it will be found, that 30 is to 2596 rather more than 23 to 1972, ſo that ſeven more tappings were at leaſt neceſſary in the winter than in the ſummer. In the months of March and November ſhe oftener underwent the operation than in any other. In theſe calculations the three months in 1783 are not included, as the year was not finiſhed.

If we compare the famous caſe of Lady PAGE, related by Dr. MEAD, the quantity of water taken from her ladyſhip appears ſmall when oppoſed to the number of pints drawn from SARAH KIPPUS. The one loſt 1920, the other 6631. It muſt be confeſſed, however, that Lady PAGE collected faſter than the poor woman whoſe caſe I have related.

I come now to ſpeak of the diſſection, and to make ſome obſervations on the whole. On the 10th of Auguſt 1783, the poor woman died; and the following day Dr. DACK, an eminent phyſician of this place, accompanied me to open the body. I firſt drew off 78 pints of clear water: ſuppoſing, therefore, all the water to have been taken away at the laſt operation, then in three weeks ſhe had collected 78 pints, which is more than three pints and a half in each day: a quantity far exceeding what ſhe had taken. I then opened into the cavity from which the water came, and ſeparated the ſac from the peritoneum, and found the ſac had ariſen in the ovarium of the left ſide. After this, I diſſected out the uterus, with the right ovarium in a natural ſtate, and thus obtained every part neceſſary to ſhow the diſeaſe, *viz.* the uterus, the right ovarium ſound, and the left enlarged into an immenſe pouch. The cyſt itſelf was not very thick, but lined in almoſt every part of it, but more eſpecially in the fore part, with ſmall oſſifications. The peritoneum was *prodigiouſly* thickened, and thus, by its additional ſtrength, became the chief ſupport of the water. There was ſomething ſingular in the ſac itſelf, for it was rather two than one, from there being an opening in the ſide of what appeared at firſt the only cavity, which led to another cavity, almoſt equally large with the firſt, ſo that if all the water in any operation had not been evacuated, it muſt probably have been owing to a difficulty in its paſſage from the ſecond into the firſt or more external cyſt. From the ſize, however, of the poor woman after each operation, it is evident, that in her there being two ſacs did not prevent the total drawing off of the water. The other viſcera appeared all in a natural ſtate. The inteſtines were quite empty, and puſhed up under the ribs, ſo as to have left but very little room for the expanſion of the lungs within the thorax. The bladder was contracted, or rather I ſhould ſay appeared leſſened. The kidneys were healthy, and both ureters in a natural ſtate. The ſac is in the collection of JOHN HUNTER, eſq.

In reflecting upon this caſe, an obvious queſtion ariſes; from whence proceeded this immenſe collection of water? At different periods of this poor woman's life the quantity drawn off, without conſidering the urine ſhe made, was much greater than the fluids ſhe drank, which appeared from meaſuring whatever ſhe took. It appears then pretty certain, that this ſuperabundant quantity muſt have been taken into the body by abſorption; and if we allow the bodies of animals to have this power of abſorbing, which we very well know vegetables are poſſeſſed of, it will account for many appearances in the animal œconomy. This poor woman collected faſter in the wet moiſt months of winter, than in ſummer.

From all, this happy concluſion may be drawn, that although human art is at preſent inſufficient to the perfect cure of diſeaſes ſimilar to the poor woman's caſe I have related, yet nature is continually defending herſelf from ſudden death; and ſuch relief *may* be granted as to protract life a long time without much pain, and often with intervals of great eaſe and comfort.

XL. *Methodus inveniendi Lineas Curvas ex proprietatibus Variationis Curvaturæ.* *Auctore* Nicolao Landerbeck, *Matheſ. Profeſſ. in Acad. Upſalienſi Adjuncto.* *Communicated by* Nevil Maſkelyne, *D. D. F. R. S. and Aſtronomer Royal.*

Read July 1, 1784.

PARS SECUNDA*.

CURVAS, ex proprietate variationis curvaturæ invenire, indice per functionem coordinatarum cujuſdam expreſſo, problema etſi indeterminatum eſt; juvat tamen ad curvas cognoſcendas, quum facile et ſponte ſeſe offerunt conditiones determinantes qui rei conveniunt et quæ in caſu quovis examini ſubjecto locum habent. Quo conſilio et qua arte calculum inire oporteat, ut et hæc et his affinia peragenda ſint, quæ ad curvas ex curvaturæ variatione cognoſcendas pertineant, per theoremata quæ ſequuntur, exponere conabor.

THEOREMA I. (Vide tab. XXI. fig. 2.)

Si curvæ cujuſdam LC index variationis curvaturæ ſit T, radius curvedinis R, ſinus anguli BCD p, poſito ſinu toto 1, arcus curvæ LC z coordinatæ perpendiculares x et y earumque fluxiones dp, dz, dx, et dy dicantur, erit $\frac{dz}{\int T dz} = -\frac{dp}{\sqrt{1-p^2}}$.

Quoniam $dx = -R dp$ et $dz = -\frac{dx}{\sqrt{1-p^2}}$ habetur $\frac{dz}{R} = -\frac{dp}{\sqrt{1-p^2}}$

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* See Vol. LXXIII. p. 456.

et quum $dR = Tdz$ erit $R = \int Tdz$ et ſubſtitutione $\frac{dz}{\int Tdz} = -\frac{dp}{\sqrt{1-p^2}}$.

Cor. 1. Hinc obtinetur $\frac{dx}{R} = -dp$, $\frac{dy}{R} = -\frac{pdp}{\sqrt{1-p^2}}$ et $\frac{dz}{R} = -\frac{dp}{\sqrt{1-p^2}}$.

Cor. 2. Si Tangens anguli BCD per r, Secans per s deſignentur habetur $\frac{dz}{\int Tdz} = -\frac{dr}{1+r^2}$ et $\frac{dz}{\int Tdz} = -\frac{ds}{s\sqrt{s^2-1}}$.

Schol. 1. Ex hoc theoremate facilis deducitur methodus generaliter calculandi variationem curvaturæ curvæ cujuſcumque. Nam $\int Tdz = -\frac{dz\sqrt{1-p^2}}{dp}$, quantitas vero $\frac{dz\sqrt{1-p^2}}{dp}$ datur, data inter x et y relatione. Sit valor quantitatis $-\frac{dz\sqrt{1-p^2}}{dp} = Z$ functioni curvæ z, $\int Tdz = Z$ et ſumtis fluxionibus $Tdz = \dot{Z}dz$ qua $T = \dot{Z}$ functioni ipſius z. Si valor quantitatis $-\frac{dz\sqrt{1-p^2}}{dp} = P$ per p expreſſus, erit $\int Tdz = P$ ſumtiſque fluxionibus $Tdz = \dot{P}dp$ et $T = \frac{\dot{P}dp}{dz}$, quæ functio eſt quantitatis p, in poteſtate ſemper eſt $\frac{dp}{dz}$ per p exprimere.

Schol. 2. Hujus etiam theorematis ſubſidio inveniri poſſunt curvæ ex data relatione inter T et z, R et z, R et y, et R et p. Si enim ſit $T = Z$ functioni quantitatis z, erit $\int Tdz = \int Zdz + A$, vi theorematis $\frac{dz}{\int Zdz + A} \left(= \frac{dz}{\int Tdz}\right) = -\frac{dp}{\sqrt{1-p^2}}$ et integratione $\int \frac{dz}{\int Zdz + A} + C = -\frac{dp}{\sqrt{1-p^2}}$. Poſita $\int \frac{dz}{\int Zdz + A} + C = b$ et N numerus cujus logarithmus hyperbolicus 1 habetur $\sqrt{1-p^2} = \frac{N^{b\sqrt{-1}} - N^{-b\sqrt{-1}}}{2\sqrt{-1}}$ et $p = \frac{N^{b\sqrt{-1}} + N^{-b\sqrt{-1}}}{2}$, quæ functiones ſunt quantitatis z, quibus poſitis $\dot{Z}$ et $\sqrt{1-\overset{|}{Z}^2}$ reſpective proveniunt $x\left(= \int dz\sqrt{1-p^2}\right) = \int \dot{Z}dz$ et $y\left(= \int pdz\right) = \int dz\sqrt{1-\overset{|}{Z}^2}$ quarum alterutra curvarum indoles innoteſcit.

Si $R = X$ functioni abſciſſæ x provenit $\frac{dx}{X}\left(=\frac{dx}{R}\right) = -dp$ et integratione $\overset{|}{X}\left(= C - \int\frac{dx}{R}\right) = p$ unde $\sqrt{1-p^2} = \sqrt{1-\overset{|}{X}^2}$ et $y\left(=\int\frac{pdx}{\sqrt{1-p^2}}\right) = \int\frac{\overset{|}{X}dx}{\sqrt{1-\overset{|}{X}^2}}$ æquatio curvæ indolem exprimens.

Et ſi $R = Y$ functioni ordinatæ y, habetur $\frac{dy}{Y}\left(=\frac{dy}{R}\right) = -\frac{pdp}{\sqrt{1-p^2}}$ et integratione $\overset{|}{Y}\left(= \int\frac{dy}{Y} + C\right) = \sqrt{1-p^2}$, unde $p = \sqrt{1-\overset{|}{Y}^2}$ et $x\left(=\int\frac{dy\sqrt{1-p^2}}{p}\right) = \int\frac{\overset{|}{Y}dy}{\sqrt{1-\overset{|}{Y}^2}}$ quæ exprimit naturam curvæ.

Hinc colligitur quod quoties Tdz perfecte integretur et $\int\frac{dz}{\int Zdz + A}$ obtineatur per arcus circulares dum aut $\int\dot{Z}dz$ aut $\int dz\sqrt{1-\overset{|}{Z}^2}$ abſolutam admittat integrationem, curvæ erunt rectificabiles, et algebraicæ, ſi relatio inter x et z vel inter y et z in relationem algebraicam inter x et y permutari poſſit.

Evidens etiam eſt quod ſi X functio eſt algebraica quantitatis x vel Y quantitatis y, et non ſolum $\frac{dx}{X}$ vel $\frac{dy}{Y}$ ſed etiam $\frac{\overset{|}{X}dx}{\sqrt{1-\overset{|}{X}^2}}$ vel $\frac{\overset{|}{Y}dy}{\sqrt{1-\overset{|}{Y}^2}}$ quantitates perfecte integrabiles, curvæ evadunt algebraicæ, alias tranſcendentes.

Exempl. 1. Invenienda ſit curva ubi variatio curvaturæ $T=\frac{3\cdot\overline{8a+27z}|^{\frac{2}{3}}-2a^{\frac{2}{3}}}{a^{\frac{1}{3}}\sqrt{\overline{8a+27z}|^{\frac{2}{3}}-4a^{\frac{2}{3}}}}$. Ut ſimplicior reddatur calculus ponatur $\overline{8a+27z}|^{\frac{2}{3}}=u$ et $a^{\frac{2}{3}}=b$ erit $z=\frac{u^{\frac{3}{2}}-8b^{\frac{3}{2}}}{27}$, $dz=\frac{du\sqrt{u}}{18}$, $T=\frac{3u-2b}{\sqrt{b}\sqrt{u-4b}}$ et $\int T dz=\frac{u\sqrt{u}\sqrt{u-4b}}{18\sqrt{b}}+A$; ſit conſtans hæc $A=a$, quod accidit evaneſcente $\int T dz\, u=b$, habetur per theorema $\frac{du\sqrt{b}}{u\sqrt{u-4b}}\left(=\frac{dz}{\int T dz}\right)=-\frac{dp}{\sqrt{1-p^2}}$ et integratione $\int\frac{du\sqrt{b}}{u\sqrt{u-4b}}+C=-\int\frac{dp}{\sqrt{1-p^2}}$, cujus æquationis termini quum ſint arcus circulares quorum ſinus $\sqrt{1-p^2}=\frac{\sqrt{u-4b}}{\sqrt{u}}$ et coſinus $p=\frac{2\sqrt{b}}{\sqrt{u}}$, poſito arcu conſtanti $C=0$, obtinetur $y\ (=\int p dz)=\int\frac{du\sqrt{b}}{9}+B)=\frac{\sqrt{u-4b}\sqrt{b}}{9}$ nam $B=\frac{4b\sqrt{b}}{9}$, poſita $y=0$ et $u=4b$, atque $x(=\int dz\sqrt{1-p^2})=\int\frac{du\sqrt{u-4b}}{18}=\frac{\overline{u-4b}|^{\frac{3}{2}}}{27}$ quibus æquationibus exterminata u et ſubſtituta a habetur $y^3=ax^2$ æquatio pro parabola cubica.

Exempl. 2. Si ſit variatio curvaturæ $T=\frac{2z}{a}$ erit $\int T dz\ \left(=\int\frac{2z dz}{a}\right)=\frac{z^2}{a}+A$ et ſi $Z=0$ $\int T dz=a$ erit conſtans $A=a$, atque vi theorematis $\frac{a dz}{a^2+z^2}\left(=\frac{dz}{\int T dz}\right)=-\frac{dp}{\sqrt{1-p^2}}$ et integratione $\int\frac{a dz}{a^2+z^2}+C=-\int\frac{dp}{\sqrt{1-p^2}}$; poſito arcu conſtanti $C=0$ cæteri ſunt æquales eorumque ſinus et coſinus, unde $\sqrt{1-p^2}=\frac{z}{\sqrt{a^2+z^2}}$, $p=\frac{a}{\sqrt{a^2+z^2}}$ et $dx\ (=dz\sqrt{1-p^2})=\frac{z dz}{\sqrt{a^2+z^2}}$ et $dy\ (=$ $p dz)$

$pdz) = \frac{adz}{\sqrt{a^2+z^2}}$, quibus conſtat curvam eſſe cãtenariam.

Exempl. 3. Sit variatio curvaturæ $T = \frac{a-z}{\sqrt{2az-z^2}}$, evadit $\int T dz = \sqrt{2az-z^2}$, per theorema $\frac{dz}{\sqrt{2az-z^2}} \left(= \frac{dz}{\int T dz}\right) = -\frac{dp}{\sqrt{1-p^2}}$ et per integrationem $\int \frac{dz}{\sqrt{2az-z^2}} + C = -\int \frac{dp}{\sqrt{1-p^2}}$, ſi arcus ille conſtans $C = 0$, cæteri ſunt æquales eorumque ſinus et coſinus, quo $\sqrt{1-p^2} = \frac{\sqrt{2az-z^2}}{a}$, $p = \frac{a-z}{a}$ et $y \left(= \int p dz\right) = \int \frac{\overline{a-z}\, dz}{a}\Big) = \frac{2az-z^2}{a}$ æquatio pro cycloide ordinaria.

THEOREMA II.

Manentibus antea adhibitis denominationibus erit $\frac{dx}{y + \int T dx} = -\frac{dp}{\sqrt{1-p^2}}$.

Quoniam $\frac{dx}{R} = -dp$, erit dividendo per $\sqrt{1-p^2}$, $\frac{dx}{R\sqrt{1-p^2}} = -\frac{dp}{\sqrt{1-p^2}}$. Propter $1 : \sqrt{1-p^2} :: CD(R) : CF = R\sqrt{1-p^2}$, ſed $dz : dx :: Tdz : Tdx$, quæ fluxio eſt ipſius DE, quare $DE = \int T dx$, unde $CF = y + \int T dx$ qua pro $R\sqrt{1-p^2}$ ſubſtituta, prodit $\frac{dx}{y + \int T dx} = -\frac{dp}{\sqrt{1-p^2}}$.

Cor. 1. Quantitas $dy + T dx$ ſemper eſt perfecte integrabilis. Nam $T dx = -\frac{ddx\sqrt{1-p^2}}{dp}$ et $dy = \frac{p dx}{\sqrt{1-p^2}}$ unde $dy + T dx = \frac{p dx}{\sqrt{1-p^2}} - \frac{ddx\sqrt{1-p^2}}{dp}$ et integratione $y + \int T dx = -\frac{dx\sqrt{1-p^2}}{dp}$.

Cor. 2. Dicatur ſemichorda curvaturæ CF F, obtinetur $\frac{dx}{\mathrm{F}} = \frac{dp}{\sqrt{1-p^2}}$, $\frac{dy}{\mathrm{F}} = -\frac{p\,dp}{1-p^2}$ et $\frac{dz}{\mathrm{F}} = -\frac{dp}{1-p^2}$.

Cor. 3. Si Tangens anguli BCD per r, Secans per s deſignentur habetur $\frac{dx}{y+\int \mathrm{T}dx} = -\frac{dr}{1+r^2}$ et $\frac{dx}{y+\int \mathrm{T}ax} = -\frac{ds}{s\sqrt{s^2-1}}$.

Schol. 1. Per hoc theorema via etiam patet calculandi generaliter variationem curvaturæ. Eſt enim $y+\int \mathrm{T}dx = -\frac{dx\sqrt{1-p^2}}{dp}$, quantitas vero $\frac{dx\sqrt{1-p^2}}{dp}$ datur data inter x et p relatione. Sit valor quantitatis $-\frac{dx\sqrt{1-p^2}}{dp} = \mathrm{X}$ functioni abſciſſæ x æquatione ad curvam inventus, erit $\int \mathrm{T}\,dx = \mathrm{X} - y$ et ſumtis fluxionibus $\mathrm{T}dx = \overset{\mathrm{I}}{\mathrm{X}}dx - dy$, qua $\mathrm{T} = \overset{\mathrm{I}}{\mathrm{X}} - \frac{dy}{dx}$ ubi tam $\overset{\mathrm{I}}{\mathrm{X}}$ quam $\frac{dy}{dx}$ ſunt functiones abſciſſæ x. Si valor quantitatis $-\frac{dx\sqrt{1-p^2}}{dp} = \mathrm{P}$ per p expreſſus, erit $\int \mathrm{T}dx = \mathrm{P} - y$ ſumtiſque fluxionibus $\mathrm{T}dx = \overset{\mathrm{I}}{\mathrm{P}}dp - dy$, qua $\mathrm{T} = \frac{\mathrm{P}dp}{dx} - \frac{p}{\sqrt{1-p^2}}$ ubi $\frac{\overset{\mathrm{I}}{\mathrm{P}}dp}{dx}$ functio eſt quantitatis p, nam $\frac{dp}{dx}$ per p exprimi poteſt.

Schol. 2. Hoc adhibito theoremate inveniri etiam poſſunt curvæ, ex data relatione inter T et x, F et x, F et y, F et z, et F et p. Poſita enim T functione quantitatis x, patet per curvarum quadraturas, aut perfectam aut imperfectam quantitatis $\mathrm{T}dx$ obtineri integrationem. Sit $\int \mathrm{T}dx = \overset{\mathrm{I}}{\mathrm{X}} + \int \overset{\mathrm{I}}{\mathrm{X}}dx$ functioni vel algebraicæ vel ex parte tranſcendenti ipſius x, cujus terminis homogeneus valor ipſius $y = \int \overset{\mathrm{II}}{\mathrm{X}}dx$ capiatur, iſque ejus indolis ut $\int \overline{\overset{\mathrm{I}}{\mathrm{X}} + \overset{\mathrm{II}}{\mathrm{X}}}dx$, vel quod idem eſt $y + \int \mathrm{T}dx =$

$X + \int \overline{\overset{\prime}{X} + \overset{\prime\prime}{X}}\,dx$ integratione abſoluta habeatur, permanente $T dz = T dx \sqrt{1 - \overset{\prime}{X}^2}$ perfecte integrabili. Per theorema deinde habetur $\frac{dx}{X + \int \overline{X + X dx}}$ $\left(= \frac{dx}{y + \int T dx}\right) = -\frac{dp}{\sqrt{1-p^2}}$, et per integrationem $\int \frac{dx}{X + \int X + \overset{\prime}{X} dx} + C = -\int \frac{dp}{\sqrt{1-p^2}}$, ſi ponatur $\int \frac{dx}{X + \int \overset{\prime}{X} + \overset{\prime}{X} dx} + C = k$ et N baſi logarithmorum hyperbolicorum, erit $\sqrt{1-p^2} = \frac{N^{k\sqrt{-1}} - N^{-k\sqrt{-1}}}{2\sqrt{-1}}$ et $p = \frac{N^{k\sqrt{-1}} + N^{-k\sqrt{-1}}}{2}$, $\sqrt{1-p^2}$ et p igitur ſunt functiones ipſius x, quæ ſi ponantur $\sqrt{1 - \overset{\prime\prime}{X}^2}$ et $\overset{\prime\prime\prime}{X}$, habetur $y \left(= \int \frac{p\,dx}{\sqrt{1-p^2}}\right) = \int \frac{\overset{\prime\prime\prime}{X} dx}{\sqrt{1 - \overset{\prime}{X}^2}}$, æquatio qua curvæ internoſcuntur.

Si ſit $F = Y$ functioni quantitatis y erit per Cor. 2. $\frac{dy}{Y} \left(= \frac{dy}{F}\right) = -\frac{dp}{1-p^2}$ et integratione $\int \frac{dy}{Y} + \log. C = \log. \sqrt{1-p^2}$, ponatur $\int \frac{dy}{Y} = k$ et N logarithmorum baſi, erit facto ad quantitates abſolutas tranſitu $CN^k = \sqrt{1-p^2}$, $p = \sqrt{1 - C^2 N^{2k}}$ et $x \left(= \int \frac{dy \sqrt{1-p^2}}{p}\right) = \int \frac{CN^k dy}{\sqrt{1 - C^2 N^{2k}}}$, æquatio quæ indolem curvæ indigitat.

Si $F = Z$ functioni ipſius z erit $\frac{dz}{Z} \left(= \frac{dz}{F}\right) = -\frac{dp}{1-p^2}$ et integratione $\int \frac{dz}{Z} + \log. C = \log. \sqrt{\frac{1-p}{1+p}}$, et ſi $\int \frac{dz}{Z} = k$ et N baſi logarithmica habetur $p = \frac{1 - C^2 N^{2k}}{1 + C^2 N^{2k}}$ et $y = \int p\,dz) = \int \frac{\overline{1 - C^2 N^{2k}}\,dz}{1 + C^2 N^{2k}}$ qua curvæ cognoſcuntur.

Conſtat hinc quod quoties $X+\int \dot{X}dx$ perfecta integratione habeatur $\int \frac{dx}{X+\int X+Xdx}$ per arcus circulares dum $\frac{\dddot{X}dx}{\sqrt{1-\dot{X}^2}}$ abſolutam admittat integrationem curva ſit algebraica, ſi vero aliter evenerit tranſcendens.

Quoties $\frac{dy}{Y}$ ſit integrale logarithmicum et $\frac{CN^k dy}{\sqrt{1-C^2N^{2k}}}$ abſolutam admittat integrationem curva eſt algebraica, in aliis caſibus tranſcendens.

Et quoties $\int \frac{dz}{Z}$ per logarithmos inveniatur, $\frac{1-C^2N^{2k}dz}{1+C^2N^{2k}}$ abſolute ſit integrabilis pariter ac $\frac{2CN^k dz}{1+C^2N^{2k}}$ curva eſt algebraica, alias tranſcendens.

Exempl. 1. Si ſit variatio curvaturæ $T=\frac{3\cdot\overline{b^2-a^2}x\sqrt{\overline{a^2-x^2}}}{a^3b}$ erit $\int Tdx$ $\left(=\frac{\overline{a^2-b^2}\cdot\overline{a^2-x^2}\sqrt{\overline{a^2-x^2}}}{a^3b}\right)=\frac{a\sqrt{\overline{a^2-x^2}}}{}-\frac{x^2\sqrt{\overline{a^2-x^2}}}{ab}-\frac{b\sqrt{\overline{a^2-x^2}}}{a}+\frac{bx^2\sqrt{\overline{a^2-x^2}}}{a^3}$, ſi ponatur $y=\frac{b\sqrt{\overline{a^2-x^2}}}{a}$ habetur $y+\int Tdx=\frac{a^4+\overline{b^2-a^2}x^2\sqrt{\overline{a^2-x^2}}}{a^3b}$, adhibendo theorema $\frac{a^3bdx}{a^4+\overline{b^2-a^2}x^2\sqrt{\overline{a^2-x^2}}}$ $\left(=\frac{dx}{y+\int Tdx}\right)=-\frac{dp}{\sqrt{1-p^2}}$ et integrando $\int\frac{a^3bdx}{a^4+\overline{b^2-a^2}x^2\sqrt{\overline{a^2-x^2}}}+C=-\int\frac{dp}{\sqrt{1-p^2}}$, cujus termini ſunt arcus circulares quorum ſinus $\sqrt{1-p^2}=\frac{a\sqrt{\overline{a^2-x^2}}}{\sqrt{a^4+\overline{b^2-a^2}x^2}}$ et coſinus $p=\frac{bx}{\sqrt{a^4+\overline{b^2-a^2}x^2}}$ evaneſcente arcu conſtanti C, quare y $\left(=\int\frac{pdx}{\sqrt{1-p^2}}\right)=\int\frac{bdx}{a\sqrt{\overline{a^2-x^2}}}\Big)=\frac{b\sqrt{\overline{a^2-x^2}}}{a}$ et in hoc caſu curva eſt ellipſis.

Exempl. 2. Sit jam variatio curvaturæ $T=\frac{2\sqrt{2ax+x^2}}{a}$ erit $\int Tdx=\frac{x\sqrt{2ax+x^2}}{a}+\int\frac{xdx}{\sqrt{2ax+x^2}}$ et poſita $y=\int\frac{adx}{\sqrt{2ax+x^2}}$ perfecta integratione habetur $y+\int Tdx=\frac{\overline{a+x}\sqrt{2ax+x^2}}{a}$. Theorematis itaque auxilio erit $\frac{adx}{\overline{a+x}\sqrt{2ax+x^2}}\left(=\frac{dx}{y+\int Tdx}\right)=-\frac{dp}{\sqrt{1-p^2}}$, et integratione $\int\frac{adx}{\overline{a+x}\sqrt{2ax+x^2}}=C=-\int\frac{dp}{\sqrt{1-p^2}}$, ſi vero arcus ille conſtans $C=0$ cæteri ſunt æquales eorumque ſinus et coſinus, unde $\sqrt{1-p^2}=\frac{\sqrt{2ax+x^2}}{a+x}$, $p=\frac{a}{a+x}$ et $y\left(=\int\frac{pdx}{\sqrt{1-p^2}}\right)=\int\frac{adx}{\sqrt{2ax+x^2}}$, æquatio indicans curvam eſſe catenariam.

THEOREMA III.

Dicatur coſinus anguli BCD q, poſito radio 1, cæteriſque manentibus denominationibus erit $\frac{dy}{\int Tdy-x}=\frac{dq}{\sqrt{1-q^2}}$.

Eſt enim $\frac{av}{R}=aq$, qua per $\sqrt{1-q^2}$ diviſa, dat $\frac{dy}{R\sqrt{1-q^2}}=\frac{dq}{\sqrt{1-q^2}}$; et ob $1:\sqrt{1-q^2}::CD\,(R):CG=R\sqrt{1-q^2}$, ſed $dz:dy::Tdz:Tdy$ cujus integrale eſt $AE=\int Tdy$, unde $CG\,(=AE-AB)=\int Tdy-x$, qua pro $R\sqrt{1-q^2}$ ſubſtituta, prodit $\frac{dy}{\int Tdy-x}=\frac{dq}{\sqrt{1-q^2}}$.

Cor. 1. Semper $Tdy-dx$ admittit perfectam integrationem. Etenim $Tdy=\frac{ddy\sqrt{1-q}}{dq}$ et $dx=\frac{qdy}{\sqrt{1-q^2}}$, quibus $Tdy-dx=\frac{ddy\sqrt{1-q}}{dq}-\frac{qdy}{\sqrt{1-q^2}}$ et integratione $\int Tdy-x=\frac{dy\sqrt{1-q^2}}{aq}$.

Cor. 2. Dicatur ſemichorda curvaturæ CG G, habetur $\frac{dy}{G} = \frac{dq}{\sqrt{1-q^2}}$, $\frac{dx}{G} = \frac{q\,dq}{1-q^2}$ et $\frac{dz}{G} = \frac{dq}{1-q^2}$.

Cor. 3. Dicatur cotangens anguli BCD t, et coſecans v erit $\frac{dy}{\int Tdy - x} = \frac{dt}{1+t^2}$ et $\frac{dy}{\int Tdy - x} = \frac{dv}{v\sqrt{v^2-1}}$.

Schol. 1. Quoniam $\int Tdy - x = \frac{dy\sqrt{1-q^2}}{dq}$ datur ex data relatione inter y et q, ſit $\frac{dy\sqrt{1-q^2}}{dq} = Y$ functioni ordinatæ y erit $\int Tdy = Y - x$ ſumtiſque fluxionibus $Tdy = \dot{Y}dy - dx$ qua $T = \dot{Y} - \frac{dx}{dy}$ functioni ipſius y. Si autem $\frac{dy\sqrt{1-q^2}}{dq} = Q$ functioni ipſius q erit $\int Tdy = Q - x$ et ſumtis fluxionibus $Tdy = \dot{Q}dq - dx$, qua habetur $T = \frac{\dot{Q}dq}{dy} - \frac{q}{\sqrt{1-q^2}}$ per q.

Schol. 2. Hujus theorematis auxilio elicere licet curvas data relatione inter T et y, G et y, G et x, G et z, et G et q. Si enim ſit T functio ipſius y generaliter $\int Tdy = Y + \int \dot{Y}dy + A$, quæ functio eſt algebraica ipſius y quoties $\int \dot{Y}dy$ abſolute ſumi poſſit. Aſſumatur $x = \int \ddot{Y}dy$, tali ipſius y functioni ut non ſolum $\int Tdy - x = Y + \int \overline{\dot{Y} + \ddot{Y}}dy$ ſed etiam $\int Tdz = \int Tdy \sqrt{1 - \dot{Y}^2}$ abſoluta integratione habeantur, provenit vi theorematis $\frac{dy}{Y + \int \overline{\dot{Y} + \ddot{Y}}dy + A} \left(= \frac{dy}{\int Tdy - x}\right) = \frac{dq}{\sqrt{1-q^2}}$ et integratione $\int \frac{dy}{Y + \int \overline{Y + \ddot{Y}}dy + A} + C = \int \frac{dq}{\sqrt{1-q^2}}$. Poſita $\frac{dy}{Y + \int \overline{\dot{Y} + \ddot{Y}}\,dy + A} + C = l$ et N baſi logarithmica erit $q = \frac{N^{l\sqrt{-1}} - N^{-l\sqrt{-1}}}{2\sqrt{-1}}$ et $\sqrt{1-q^2}$

$= \frac{N^{l\sqrt{-1}} + N^{-l\sqrt{-1}}}{2}$ quæ functiones ſunt quantitatis y, quibus poſitis $\overset{|||}{Y}$ et $\sqrt{1 - Y^2}$ prodit $x \left(= \int \frac{q\,dy}{\sqrt{1-q^2}}\right) = \int \frac{\overset{|||}{Y}\,dy}{\sqrt{1-\overset{|||}{Y}^2}}$ æquatio quæ indolem curvarum indicat.

Si $G = X$ functioni ipſius x erit per Cor. 2. $\frac{dx}{X} \left(= \frac{dx}{G}\right) = \frac{q\,dq}{1-q^2}$, et integratione log. $CN^l \left(= \int \frac{dx}{X} + \text{log. } C\right) = \text{log. } \frac{1}{\sqrt{1-q^2}}$ ſi $\int \frac{dx}{X} = l$, exinde $\sqrt{1-q^2} = \frac{1}{CN^l}$, $q = \frac{\sqrt{C^2N^{2l}-1}}{CN^l}$ et $y \left(= \int \frac{dx\sqrt{1-q^2}}{q}\right) = \int \frac{dx}{\sqrt{C^2N^{2l}-1}}$, quæ curvæ naturam indigitat.

Si $G = Z$ functioni ipſius z erit $\frac{dz}{Z} \left(= \frac{dz}{G}\right) = \frac{dq}{1-q^2}$, et integratione log. $CN^l \left(= \frac{dz}{Z} + C\right) = \text{log. } \sqrt{\frac{1+q}{1-q}}$ ſi $\int \frac{dz}{Z} = l$, unde $q = \frac{C^2N^{2l}}{1+C^2N^{2l}} \sqrt{1-q^2} = \frac{2CN^l}{1+C^2N^{2l}}$ $x \left(= \int q\,dz\right) = \int \frac{\overline{C^2N^{2l}-1}\,dz}{1+C^2N^{2l}}$ et $y \left(= \int dz\sqrt{-1q^2}\right) = \int \frac{2CN^l dz}{1+C^2N^{2l}}$ quibus curvæ cognoſcuntur.

Patet hinc quod quando $Y + \int Y dy$ algebraice habeatur $\int \frac{dy}{Y + \int \overset{|}{Y} + \overset{||}{Y} dy + A}$ per quadraturam circuli, et $\int \frac{\overset{|||}{Y}\,dy}{\sqrt{1-\overset{|||}{Y}^2}}$ etiam obtineatur algebraice, curvæ evadunt algebraicæ, ſecus vero tranſcendentes.

Quando $\int \frac{dx}{X}$ vel $\int \frac{dz}{Z}$ obtineatur per logarithmos, et $\int \frac{dx}{\sqrt{C^2N^{2l}-1}}$, vel tam $\int \frac{\overline{C^2N^{2l}-1}\,dz}{1+C^2N^{2l}}$ quam $\int \frac{2CN^l dz}{1+C^2N^{2l}}$ abſoluta integratione, curvæ erunt algebraicæ.

Exempl. 1. Sit index variationis curvaturæ $T = \frac{6y}{a}$ erit $\int T dy = \frac{3y^2}{a} + A$, ſi quantitas illa conſtans $A = \frac{a}{2}$ quod evenit quum $\int T dy = \frac{a}{2}$ et $y = 0$; ſumatur $x = \frac{y^2}{a}$ erit vi theorematis $\frac{2ady}{a^2+4y^2}$ $\left(= \frac{dy}{\int T dy - x}\right) = \frac{dq}{\sqrt{1-q^2}}$ et integratione $\int \frac{2ady}{a^2+4y^2} + C = \int \frac{dq}{\sqrt{1-q^2}}$, cujus æquationis termini quoniam ſint arcus circulares quorum ſinus $q = \frac{2y}{\sqrt{a^2+4y^2}}$ et coſinus $\sqrt{1-q^2} = \frac{a}{\sqrt{a^2+4y^2}}$, arcu conſtanti $C = 0$, obtinetur $x \left(= \int \frac{qdy}{\sqrt{1-q^2}}\right) = \frac{y^2}{a}$ æquatio pro parabola Apolloniana.

Exempl. 2. Si ſit $T = \frac{a^2}{y\sqrt{a^2-y^2}}$ habetur $\int T dy = \int \frac{dy\sqrt{a^2-y^2}}{y} - \sqrt{a^2-y^2} + A$, ſi quantitas illa conſtans $A = 0$ quod evenit quum $\int T dy = 0$ et $y = a$, et aſſumatur $x = \int \frac{dy\sqrt{a^2-y^2}}{y}$, evadit per theorema $-\frac{dy}{\sqrt{a^2-y^2}} \left(= \frac{dy}{\int T dy - x}\right) = \frac{dq}{\sqrt{1-q^2}}$, et per integrationem $-\int \frac{dy}{\sqrt{a^2-y^2}} + C = \int \frac{dq}{\sqrt{1-q^2}}$, quorum arcuum ſinus $q = \frac{\sqrt{a^2-y^2}}{a}$ et coſinus $\sqrt{1-q^2} = \frac{y}{a}$ ſi conſtans ille $C = 0$, atque inde $dx \left(\frac{qdy}{\sqrt{1-q^2}}\right) = \frac{dy\sqrt{1-y^2}}{y}$ qua patet curvam eſſe tractoriam.

THEOREMA IV.

Dicatur ſumma tangentium angulorum HCD et BCD H, et differentia tangentium angulorum HCD et CKB K, retentis reliquis denominationibus erit $\frac{dx}{\int H dx} = -\frac{dp}{\sqrt{1-p^2}}$ et $\frac{dy}{\int K dy} = \frac{dq}{\sqrt{1-q^2}}$.

Quoniam $dy = \frac{p\,dx}{\sqrt{1-p^2}}$ erit $dy + T\,dx = \overline{T + \frac{p}{\sqrt{1-p^2}}}\,dx$ et quum $H = T + \frac{p}{\sqrt{1-p^2}}$ habetur $dy + T\,dx = H\,dx$. Eodem modo quum $dx = \frac{q\,dy}{\sqrt{1-q^2}}$ erit $\int T\,dy - dx = T - \frac{q}{\sqrt{1-q^2}}\,dy$, ſed $K = T - \frac{q}{\sqrt{1-q^2}}$, unde $\int T\,dy - x = K\,dy$. Per theorema igitur 2 et 3 provenit $\frac{dx}{\int H\,dx} = -\frac{dp}{\sqrt{1-p^2}}$ et $\frac{dy}{\int K\,dy} = \frac{dq}{\sqrt{1-q^2}}$.

Cor. Si ſit ut antea tangens anguli BCD r, cotangens t, ſecans s, et coſecans v, erit $\frac{dx}{\int H\,dx} = -\frac{dr}{r+r^2}$ et $\frac{dx}{\int H\,dx} = -\frac{ds}{s\sqrt{s^2-1}}$, $\frac{dy}{\int K\,dy} = \frac{dt}{1+t^2}$ et $\frac{dy}{\int K\,dy} = \frac{dv}{v\sqrt{v^2-1}}$.

Schol. Ope hujus theorematis invenire licet curvas, data relatione inter H et x atque K et y. Itaque ſit $H = X$ functioni ipſius x erit $\int H\,dx = \int X\,dx + A$, vi theorematis $\frac{dx}{\int X\,dx + A}\left(= \frac{dx}{\int H\,dx}\right) = -\frac{dp}{\sqrt{1-p^2}}$, et integratione $\int \frac{dx}{\int X\,dx + A} + C = -\int \frac{dp}{\sqrt{1-p^2}}$. Poſita $\int \frac{dx}{\int X\,dx + A} + C = m$, et N logarithmorum baſi prodit $\sqrt{1-p^2} = \frac{N^{m\sqrt{-1}} - N^{-m\sqrt{-1}}}{2\sqrt{-1}}$ et $p = \frac{N^{m\sqrt{-1}} + N^{-m\sqrt{-1}}}{2}$, quibus functionibus quantitatis x poſitis $\sqrt{1-\overset{\prime}{X}{}^2}$ et $\overset{\prime}{X}$ provenit æquatio $y \left(= \int \frac{p\,dx}{\sqrt{1-p^2}}\right) = \frac{\overset{\prime}{X}\,dx}{\sqrt{1-\overset{\prime}{X}{}^2}}$ naturam curvarum exprimens.

Si $K = Y$ functioni quantitatis y, eadem calculandi ratione habetur $x \left(= \int \frac{q\,dy}{\sqrt{1-q^2}}\right) = \frac{\overset{\prime}{Y}\,dy}{\sqrt{1-\overset{\prime}{Y}{}^2}}$ æquatio qua curvæ cognoſcuntur.

Quando $\int Xdx$ vel $\int Ydy$ abſoluta integratione, $\int\frac{dx}{\int Xdx+A}$ vel $\int\frac{dy}{\int Ydy+A}$ per rectificationem circuli, et $\int\frac{\overset{|}{X}dx}{\sqrt{1-\overset{|}{X}^2}}$ vel $\int\frac{\overset{|}{Y}dy}{\sqrt{1-\overset{|}{Y}^2}}$ integratione perfecta obtineantur, curva eſt algebraica.

Exempl. 1. Si ſit $H=\frac{a+12x}{2\sqrt{a}\sqrt{x}}$ erit $\int Hdx=\frac{\overline{a+4x}\sqrt{x}}{\sqrt{a}}+A$, et poſita $A=0$ habetur per theorema $\frac{dx\sqrt{a}}{\overline{a+4x}\sqrt{x}}\ (=\frac{dx}{\int Hdx})=-\frac{dp}{\sqrt{1-p^2}}$ et per integrationem $\int\frac{dx\sqrt{a}}{\overline{a+4x}\sqrt{x}}+C=\int\frac{dp}{\sqrt{1-p^2}}$, cujus termini quum ſint arcus circulares quorum ſinus $\sqrt{1-p}=\frac{2\sqrt{x}}{\sqrt{a+4x}}$ et coſinus $p=\frac{\sqrt{a}}{\sqrt{a+4x}}$, poſita $C=0$, obtinetur $y\ (=\int\frac{pdx}{\sqrt{1-p^2}})=\sqrt{ax}$, quæ parabolam Apolloniam exprimit.

Exempl. 2. Sit $H=\frac{2a^4-x^4}{ax^2\sqrt{a-x^2}}$ erit $\int Hdx=\frac{\overline{x^2-2a^2}\sqrt{a^2-x^2}}{ax}+A$, et ſi $A=0$, per theorema $\frac{axdx}{\overline{x^2+2a^2}\sqrt{a^2-x^2}}\ (=\frac{dx}{\int Hdx})=-\frac{dp}{\sqrt{1-p^2}}$ et per integrationem $\int\frac{axdx}{\overline{x^2-2a^2}\sqrt{a^2-x^2}}+C=-\int\frac{dp}{\sqrt{1-p^2}}$, et ſi $C=0$, $\sqrt{1-p^2}=\frac{\sqrt{a^2-x^2}}{\sqrt{2a^2-x^2}}$ $p=\frac{a}{\sqrt{2a^2-x^2}}$ et $y\ (=\int\frac{pdx}{\sqrt{1-p^2}})=\int\frac{adx}{\sqrt{a^2-x^2}}$ æquatio pro curva ſinuum.

Exempl. 3. Si ſit $K=\frac{\overline{5a^2+6y^2}.y}{a\sqrt{a^2+y^2}}$ erit $\int Kdy=\frac{\overline{a^2+2y^2}\sqrt{a^2+y^2}}{a^2}+A$, ſi $A=0$ habetur per theorema $\frac{a^2dy}{\overline{a^2+2y^2}\sqrt{a^2+y^2}}\ (=\frac{dy}{\int Kdy})=\frac{dq}{\sqrt{1-q^2}}$ et integratione $\int\frac{a^2dy}{\overline{a^2+2y^2}\sqrt{a^2+y^2}}+C=-\int\frac{dp}{\sqrt{1-p^2}}$, qua $q=\frac{y}{\sqrt{a^2+2y^2}}$, $\sqrt{1-q^2}=\frac{\sqrt{a^2+y^2}}{\sqrt{a^2+2y^2}}$, ſi $C=0$, unde $x\ (=\int\frac{qdy}{\sqrt{1-q^2}})=\sqrt{a^2+y^2}$ æquatio pro hyperbola æquilatera.

Exempl. 4. Sit $K = \frac{y}{\sqrt{a^2 - y^2}}$ erit $\int K dy = A - \sqrt{a^2 - y^2}$ et ſi $A = 0$, per theorema $- \frac{dy}{\sqrt{a^2 - y^2}} \left(= \frac{dy}{\int K dy}\right) = \frac{dq}{\sqrt{1 - q^2}}$ et per integrationem $- \int \frac{dy}{\sqrt{a^2 - y^2}} + C = \int \frac{dq}{\sqrt{1 - q^2}}$ qua $q = \frac{\sqrt{a^2 - y^2}}{a}$, $\sqrt{1 - q^2} = \frac{y}{a}$ et $dx \left(= \frac{q dy}{\sqrt{1 - q^2}}\right) = \frac{dy \sqrt{a^2 - y^2}}{y}$ quæ Tractoriam exprimit.

THEOREMA V.

Deſignetur productum tangentium angulorum HCD et BCD per U, et angulorum HCD et CKB per V cæteris manentibus erit $\frac{dx}{\int U dx - x} = - \frac{dp}{p}$ et $\frac{dy}{Y + \int V dy} = \frac{dq}{q}$.

Quoniam $dy = \frac{p dx}{\sqrt{1 - p^2}}$ et $U = \frac{T p}{\sqrt{1 - p^2}}$ erit $T dy \left(= \frac{T p dx}{\sqrt{1 - p^2}}\right) = U dx$, et integratione $\int T dy = \int U dx$ qua $\int T dy - x = \int U dx - x$. Et quoniam $dx = \frac{q dy}{\sqrt{1 - q}}$ et $V = \frac{T q}{\sqrt{1 - q}}$ erit $T dx \left(= \frac{T q dy}{\sqrt{1 - q^2}}\right) = V dy$, $\int T dx = \int V dy$ et $y + \int T dx = y + \int V dy$. Theoremate 2. et 3. prodit $\frac{dx}{\int U dx - x} = - \frac{dp}{p}$ et $\frac{dy}{y + \int V dy} = \frac{dq}{q}$.

Cor. Si anguli BCD tangens, cotangens, &c. deſignentur ut antea, habetur $\frac{dx}{\int U dx - x} = - \frac{dr}{r \,.\, 1 + r^2}$, $\frac{dy}{y + \int V dy} = - \frac{dt}{t \,.\, 1 + t^2}$, &c.

Schol. Per hoc theorema curvæ inveniuntur ex data relatione inter U et x, atque inter V et y. Si enim ſit $U = X$ functioni ipſius x erit $\int U dx = \int X dx + A$, per theorema $\frac{dx}{\int X dx - x + A} \left(= \frac{dx}{\int U dx - x}\right) = - \frac{dp}{p}$, et per integrationem $\int \frac{dx}{\int X dx - x + A} + \log. C =$

log. $\frac{1}{p}$. Ponatur $\int \frac{dx}{\int Xdx - x + A} = n$ et N baſi logarithmica, erit $\frac{1}{p} = CN^n$, $p = \frac{1}{CN^n}$, $\sqrt{1-p^2} = \frac{\sqrt{C^2N^{2n}-1}}{CN^n}$ et $y \left(= \frac{pdx}{\sqrt{1-p^2}}\right) = \int \frac{dx}{\sqrt{C^2N^{2n}-1}}$ qua æquatione curvarum indoles innoteſcit.

Si V = Y functioni ipſius y, eadem calculandi ratione provenit $x \left(= \int \frac{qdy}{\sqrt{1-q^2}}\right) = \int \frac{CN^n dy}{\sqrt{1-C^2N^{2n}}}$ qua curvæ cognoſcuntur.

Evidens hinc eſt quod quoties $\int Xdx$ vel $\int Ydy$ algebraice $\int \frac{dx}{\int Xdx - x + A}$ vel $\int \frac{dy}{y + \int Ydy + A}$ per logarithmos, atque $\int \frac{dx}{\sqrt{C^2N^{2n}-1}}$ vel $\int \frac{CN^n dy}{\sqrt{1-C^2N^{2n}}}$ integratione abſoluta, obtineantur, curva eſt algebraica.

Exempl. 1. Si ſit $U = 3$ erit $\int Udx = 3x + A$, ſi vero $\int Udx = \frac{a}{2}$ quando $x = 0$ erit $A = \frac{a}{2}$ et $\int Udx - x = \frac{a+4x}{2}$. Per theorema igitur $\frac{2dx}{a+4x} \left(= \frac{dx}{\int Udx - x}\right) = -\frac{dp}{p}$ et per integrationem log. $\sqrt{a+4x}$ + log. C = log. $\frac{1}{p}$, poſita $p = 1$ dum $x = 0$ log. $C = -$ log. $\sqrt{a}$, unde facto a logarithmis tranſitu $\frac{\sqrt{a+4x}}{\sqrt{a}} = \frac{1}{p}$, qua $p = \frac{\sqrt{a}}{\sqrt{a+4x}}$, $\sqrt{1-p^2} = \frac{2\sqrt{x}}{\sqrt{a+4x}}$ et $y \left(= \int \frac{pdx}{\sqrt{1-p^2}}\right) = \int \frac{dx\sqrt{a}}{2\sqrt{x}}\Big) = \sqrt{ax}$ æquatio pro Parabola Apolloniana.

Exempl. 2. Sit $U = \frac{x^3 - 4a^3}{x\sqrt{x}}$ erit $\int Udx = \frac{x^3 - 2a^3}{3x^2} + A$, ſi autem $\int Udx = 0$ et $x = a\sqrt[3]{2}$, erit $A = 0$ et $\int Udx - x = \frac{2 \cdot a^3 - x^3}{3x^2}$. Vi igitur theorematis erit $\frac{3x^2dx}{2a^3 - x^3} \left(= \frac{dx}{\int Udx - x}\right) = -\frac{dp}{p}$, et integratione log. $\frac{a\sqrt{a}}{\sqrt{a^3-x^3}}$ + log. C = log. $\frac{1}{p}$ qua $p = \frac{\sqrt{a^3-x^3}}{a\sqrt{a}}$; $\sqrt{1-p^2} = \frac{x\sqrt{x}}{a\sqrt{a}}$ et $y \left(= \int \frac{p\,dx}{\sqrt{1-p^2}}\right) = \frac{dx\sqrt{a^3-x^3}}{x\sqrt{x}}$ æquatio ad curvam quæſitam.

Exempl. 3. Si $V = -\frac{1}{2}$ erit $\int V dy = A - \frac{y}{2}$, poſita $\int V dy = 0$ et $y = 0$ erit $A = 0$ et $y + \int V dy = \frac{y}{2}$. Per theorema obtinetur $\frac{2dy}{y} \left(= \frac{dy}{y + \int V dy}\right) = \frac{dq}{q}$ et per integrationem log. y^2 + log. C = log. q, ſi $q = 1$ et $y = a$ erit log. C = − log. a^2, unde $q = \frac{y^2}{a^2}$, $\sqrt{1-q^2} = \frac{\sqrt{a^4-y^4}}{a^2}$ atque $dx \left(= \frac{q\,dy}{\sqrt{1-q^2}}\right) = \frac{y^2 dy}{\sqrt{a^4-y^4}}$, curva ergo eſt Elaſtica,

Exempl. 4. Sit $V = \frac{a^2-2y^2}{y^2}$ erit $\int V dy = A - \frac{a^2+2y^2}{y}$, ſi $\int V dy = -3a$ et $y = a$ erit $A = 0$, indeque $y + \int V dy = -\frac{a^2+y^2}{y}$. Theorematis ope habetur $-\frac{y\,dy}{a^2+y^2} \left(= \frac{dy}{y + \int V dy}\right) = \frac{dq}{q}$ et integratione log. $\frac{1}{\sqrt{a^2+y^2}}$ + log. C = log. q, ſi $q = 1$ et $y = 0$ erit log. C = log. a et exinde $q = \frac{a}{\sqrt{a^2+y^2}}$, $\sqrt{1-q^2} = \frac{y}{\sqrt{a^2+y^2}}$ et $dx \left(= \frac{q\,dy}{\sqrt{1-q^2}}\right) = \frac{a\,dy}{y}$ æquatio pro Logarithmica.

THEOREMA VI.

Dicatur ED L, et AE M, retentis præterea adhibitis denominationibus erit $\frac{dL}{T} = dx$ et $\frac{dM}{T} = dy$.

Quoniam $dz : dx :: Tdz$ $(dR) : Tdx$ habetur $dL = Tdx$ et

$\frac{dL}{T} = dx$. Et quoniam $dz : dy :: Tdz\ (dR) : Tdy$ obtinetur $dM = Tdy$ et $\frac{dM}{T} = dy$.

Cor. Quum $Tdy = Udx$ et $Tdx = Vdy$, erit ſubſtitutione $\frac{dM}{U} = dx$ et $\frac{dL}{V} = dy$.

Schol. Hoc adhibito theoremate inveniri poſſunt curvæ data relatione inter T et L, T et M, atque inter U et M et V et L. Ponatur $L = \overset{\prime}{T}$ functioni quantitatis T habetur per theorema $\frac{d\overset{\prime}{T}}{T}\ (= \frac{dL}{T}) = dx$ et integratione $\int \frac{d\overset{\prime}{T}}{T} + C = x$ qua T per x datur. Curvæ deinde per theorema 2. elici poſſunt.

Si $M = \overset{\prime}{T}$ ipſius T functioni, habetur eodem modo T per y. Si $M = \overset{\prime}{U}$ functioni ipſius U, obtinetur U per x, et ſi $L = \overset{\prime}{V}$ functioni quantitatis V, datur V per y. Per theorema deinde 3. et 5. curvæ inveniuntur.

Evidens quidem eſt quod curvæ eſſe non poſſunt algebraicæ niſi $\int \frac{dL}{T}$, $\int \frac{dM}{T}$, $\int \frac{dM}{U}$ vel $\int \frac{dL}{V}$, obtineantur integratione abſoluta.

Exempl. 1. Si ſit $L = \frac{aT^3}{54}$ erit $dL = \frac{aT^2 dT}{18}$, et per hoc theorema $\frac{aTdT}{18}\ (= \frac{dL}{T}) = dx$ et integratione $\frac{aT^2}{36} + C = x$ qua $T = \frac{6\sqrt{x}}{\sqrt{a}}$, ſi $C = 0$. Per theorema 2. reperitur $y = \sqrt{ax}$, æquatio pro Parabola Apolloniana.

Exempl. 2. Si ſit $M = -\int \frac{aT^2 dT}{2\cdot\overline{1+T^2}|^2}$ erit $dM = -\frac{aT^2 dT}{2\cdot\overline{1+T}|^2}$ et ope theorematis $-\frac{aTdT}{2\cdot\overline{1+T^2}|^2}\ (= \frac{dM}{T}) = dy$, et integratione $\frac{a}{4\cdot 1+T^2} + C = y$, qua ſi $C = 0$, $T = \frac{\sqrt{a-4y}}{2\sqrt{y}}$. Per theorema 3. habetur

$dx = \frac{2dy\sqrt{y}}{\sqrt{a-4y}}$, æquatio pro Cycloide ordinaria.

Exempl. 3. Sit $L = -a\sqrt{V}$ erit $dL = -\frac{adV}{2\sqrt{V}}$ et per theorema $-\frac{adV}{2V\sqrt{V}} \left(= \frac{dL}{V}\right) = dy$ et integratione $\frac{a}{\sqrt{V}} + C = y$, et ſi $C = 0$, habetur $V = \frac{a^2}{y^2}$ et deinde per theorema 5. $dx = \frac{dy\sqrt{a^2-y^2}}{y}$, qua conſtat curvam eſſe Tractoriam.

THEOREMA VII.

Dicatur ut antea CF F et CG G, et ſumma tangentium angulorum HCD et BCD, H, et differentia tangentium angulorum HCD et CKB, K, erit $\frac{dF}{H} = dx$ et $\frac{dG}{K} = dy$.

Quoniam dF $(= dy + Tdx) = Hdx$ et dG $(= \int Tdy - x) = Kdy$ provenit $\frac{dF}{H} = dx$ et $\frac{dG}{K} = dy$.

Cor. Quum $F = -\frac{dx\sqrt{1-p^2}}{dp}$ et $G = \frac{dy\sqrt{1-q^2}}{dp}$ provenit diviſione $\frac{dF}{FH} = -\frac{dp}{\sqrt{1-p^2}}$ atque $\frac{dG}{GK} = \frac{dq}{\sqrt{1-q^2}}$.

Schol. Auxilio hujus theorematis inveniuntur curvæ ex data relatione inter F et H, G et K, H et p atque K et q. Nam ſi ſit $F = \overset{\prime}{H}$ functioni ipſius H, vel $G = \overset{\prime}{K}$ functioni ipſius K, habetur per theorema $\frac{d\overset{\prime}{H}}{H} \left(= \frac{dF}{H}\right) = dx$ et integratione $\int \frac{d\overset{\prime}{H}}{H} + C = x$ qua H per x datur. Eodem modo $\frac{d\overset{\prime}{K}}{K} \left(= \frac{dG}{K}\right) = dy$ et integratione $\int \frac{d\overset{\prime}{K}}{K} + C = y$ qua K per y obtinetur. Theorema 4. ulterius progredienti viam monſtrat ad curvas inveniendas.

Patet quod curva non ſit algebraica niſi $\int\frac{dH}{H}$ vel $\int\frac{dK}{K}$ obtineantur perfecta integratione.

Exempl. 1. Si ſit $F=\frac{a}{\sqrt{1+H^2}}$ habetur per theorema $-\frac{adH}{\overline{1-H^2}\vert^{\frac{3}{2}}}\left(=\frac{dF}{H}\right)=dx$, et integratione $\frac{aH}{\sqrt{1-H^2}}+C=-x$ qua $H=-\frac{x}{\sqrt{a^2-x^2}}$, poſita $C=0$. Per theorema deinde 4. provenit $y=\sqrt{a^2-x^2}$ æquatio pro circulo.

Exempl. 2. Sit $F=\frac{a\cdot\overline{H^3+H^2+6\sqrt{H^2-12}}}{108}$, erit per theorema $\frac{a\cdot\overline{H^2-6+H\sqrt{H^2-12}}\cdot dH}{36\sqrt{H^2-12}}\left(=\frac{dF}{H}\right)=dx$ et integratione facta $\frac{a\cdot\overline{H^2-6+H\sqrt{H^2-12}}}{72}+C=x$, et poſita $C=0$ habetur $H=\frac{a+12x}{2\sqrt{a}\sqrt{x}}$, unde per theorema 4. prodit $y=\sqrt{ax}$ æquatio pro Parabola Apolloniana.

Exempl. 3. Sit $G=-\frac{a\cdot\overline{4+K^2}}{4}$ erit per theorema $\frac{adK}{2}\left(=\frac{dG}{K}\right)=dy$, et integratione $\frac{aK}{2}+C=y$, et ſi $C=0$ $K=\frac{2y}{a}$ unde per theorema 4. $dx=\frac{ady}{y}$, qua conſtat curvam eſſe Logarithmicam.

THEOREMA VIII.

Dicatur ut antea productum tangentium angulorum HCD et BCD U, et productum tangentium angulorum HCD et CKB V manentibus reliquis denominationibus erit $\frac{dG}{U-1}=dx$ et $\frac{dF}{1+V}=dy$.

Quoniam $G=\int T\,dy-x$ erit $dG=T\,dy-dx$, ſed $T\,dy=U\,dx$, unde $dG=\overline{U-1}\,dx$ et $\frac{dG}{U-1}=dx$. Eodem modo quum $F=y+\int T\,dx$ erit $dF=dy+T\,dx$, ſed $T\,dx=V\,dy$ quare $dF=\overline{1+V}\,dy$ et $\frac{dF}{1+V}=dy$.

Cor. Quoniam $G=\frac{dy\sqrt{1-q^2}}{dq}$ et $F=-\frac{dx\sqrt{1-p^2}}{dp}$, habetur ſubſtitutione debita $\frac{dG}{G\,.\,\overline{U-1}}=-\frac{dp}{p}$ et $\frac{dF}{F^2\,\overline{1+V}}=\frac{dq}{q}$.

Schol. Ope hujus theorematis indagantur curvæ data relatione inter G et U vel inter F et V. Nam ſi ſit $G=\overset{\prime}{U}$ functioni quantitatis U vel $F=\overset{\prime}{V}$ functioni quantitatis V obtinetur per theorema in caſu priori $\frac{d\overset{\prime}{U}}{U-1}\left(=\frac{dG}{U-1}\right)=dx$ et integratione $\int\frac{d\overset{\prime}{U}}{U-1}+C=x$, qua U per x habetur; in poſteriori $\frac{d\overset{\prime}{V}}{1+V}\left(=\frac{dF}{1+V}\right)=dy$ et integratione $\int\frac{d\overset{\prime}{V}}{1+V}+C=y$, qua V habetur per y. Per theorema deinde 5. curvæ cognoſcuntur.

Datur etiam per Cor. U in p, et V in q, et conſequenter T in p vel q, nam $U=\frac{Tp}{\sqrt{1-p^2}}$ et $V=\frac{Tq}{\sqrt{1-q^2}}$.

Conſtat hinc quod curvæ non ſint algebraicæ niſi $\int\frac{d\overset{\prime}{U}}{U-1}$ vel $\int\frac{d\overset{\prime}{V}}{1+V}$ obtineantur integratione abſoluta.

Exempl. 1. Si ſit $G=\frac{a\,.\,\overline{U-3}}{2}$ erit per theorema $\frac{a\,dU}{2\,\overline{U-1}}\left(=\frac{dG}{U-1}\right)=dx$ et integratione log. $1-U+$ log. $C=\frac{2x}{a}$ et ſi $C=\frac{a^2}{2}$

log. $\frac{a^2 . \overline{1-U}}{2} = \frac{2x}{a}$ et $\frac{a . \overline{1-U}}{2} = N^{\frac{2x}{a}}$ qua $U = \frac{a^2 - 2N^{\frac{2x}{a}}}{a^2}$. Per theorema deinde 5. habetur $dy = \frac{dxN^{\frac{x}{a}}}{a}$ qua conſtat curvam eſt Logarithmicam.

Exempl. 2. Si ſit $T = \frac{a . \overline{V-1}\sqrt{V+2}}{3\sqrt{3}}$ erit per theorema $\frac{adV}{2\sqrt{3}\sqrt{V+2}} \left(= \frac{dF}{1+V}\right) = dy$ et per integrationem $\frac{a\sqrt{V+2}}{\sqrt{3}} = y$ qua $V = \frac{3y^2 - 2a^2}{a^2}$; et per theorema 5. $dx = \frac{dy\sqrt{y^2-a^2}}{a}$, æquatio ad curvam cujus conſtructio a quadratura hyperbolæ dependet.

THEOREMA IX.

Sint LC et lc duæ curvæ eandem habentes Evolutam QD, dicatur radiorum oſculi CD cD conſtans differentia cC b, curvæ lc variatio curvaturæ S, ceteriſque ut antea manentibus erit $\frac{dR}{R-bS} = -\frac{dp}{\sqrt{1-p^2}}$.

Quoniam radius curvaturæ DH evolutæ ſit $RT = R - bS$, erit $\frac{1}{R-bS} = \frac{1}{RT}$, quæ per $dR\ (= Tdz) = -\frac{RTdp}{\sqrt{1-p^2}}$ multiplicata, moſtrat eſſe $\frac{dR}{R-bS} = -\frac{dp}{\sqrt{1-p^2}}$.

Cor. Si ſint ut antea tangens anguli BCD r et ſecans s, habetur $\frac{dR}{R-bS} = -\frac{dr}{1+r^2}$ et $\frac{dR}{R-bS} = -\frac{ds}{S\sqrt{S^2-1}}$.

Schol. Subſidio hujus theorematis invenire licet curvas, data relatione inter S et R vel inter S et T nam $\frac{S}{T} = \frac{R}{R-b}$. Itaque ſi ponatur $S = \dot{R}$ functioni radii curvedinis R, erit $\frac{dR}{R - b\dot{R}} \left(= \frac{dR}{R - bS}\right)$ $= -\frac{dp}{\sqrt{1-p^2}}$, et integratione $\int \frac{dR}{R - b\dot{R}} + C = -\int \frac{dp}{\sqrt{1-p^2}}$. Sit $\int \frac{dR}{R - b\dot{R}} + C = f$ et N logarithmorum baſi habetur $\sqrt{1-p^2} = \frac{N^{f\sqrt{-1}} - N^{-f\sqrt{-1}}}{2\sqrt{-1}}$ et $p = \frac{N^{f\sqrt{-1}} + N^{-f\sqrt{-1}}}{2}$ functionibus quantitatis R, quibus R per p exprimi poteſt. Per theorema igitur 1. curvas internoſcere valemus.

Si $R = \dot{S}$ functioni quantitatis S habetur $\frac{d\dot{S}}{\dot{S} - bS} \left(= \frac{dR}{R - bS}\right)$ $= -\frac{dp}{\sqrt{1-p^2}}$, et integratione $\int \frac{d\dot{S}}{\dot{S} - bS} + C = -\int \frac{dp}{\sqrt{1-p^2}}$, poſita $\int \frac{d\dot{S}}{\dot{S} - bS} + C = g$, erit $\sqrt{1-p^2} = \frac{N^{g\sqrt{-1}} - N^{-g\sqrt{-1}}}{2 - \sqrt{1}}$ et $p = \frac{N^{g\sqrt{-1}} + N^{-g\sqrt{-1}}}{2}$ quibus S per p datur. Per theoremata Partis I. invenire licet curvas omnes eandem evolutam habentes.

Hinc videtur, quod curvæ non ſint algebraicæ niſi $\int \frac{dR}{R - b\dot{R}}$ vel $\int \frac{d\dot{S}}{\dot{S} - bS}$ per circuli rectificationem obtineatur.

Exempl. 1. Si ſit $S = \frac{2R}{\sqrt{a} \cdot \sqrt{R-a}}$ ſuppoſita $b = a$, erit per theorema $\frac{dR\sqrt{a}}{2R\sqrt{R-a}} \left(= \frac{dR}{R - bS}\right) = -\frac{dp}{\sqrt{1-p^2}}$ et integratione $\int \frac{dR\sqrt{a}}{2R\sqrt{R-a}} + C = -\int \frac{dp}{\sqrt{1-p^2}}$, ſi vero arcus ille conſtans $C = 0$ erit $\sqrt{1-p} = \frac{\sqrt{R-a}}{\sqrt{R}}$ qua $R = ap^2$, et per Cor. 1. Theor. 1. habetur $dy = \frac{adx}{\sqrt{x^2 - a^2}}$, æquatio pro Catenaria.

Exempl. 2. Sit $S = \frac{5a^2+R^2}{a \cdot a-5R}$, poſita $b = \frac{a}{5}$ erit per theorema, $-\frac{adR}{a^2+R^2}\left(= \frac{dR}{R-bS}\right) = -\frac{dp}{\sqrt{1-p^2}}$ et facta integratione $-\int\frac{adR}{a^2+R^2}+C = -\int\frac{dp}{\sqrt{1-p^2}}$, qua ſi $C=0$, habetur $\sqrt{1-p^2} = \frac{R}{\sqrt{a^2+R^2}}$ et $R = \frac{a\sqrt{1-p^2}}{p}$. Per theorema 1. $dx = \frac{dy\sqrt{a^2-y^2}}{y}$ qua conſtat curvam eſſe Tractoriam.

Fig. 1.

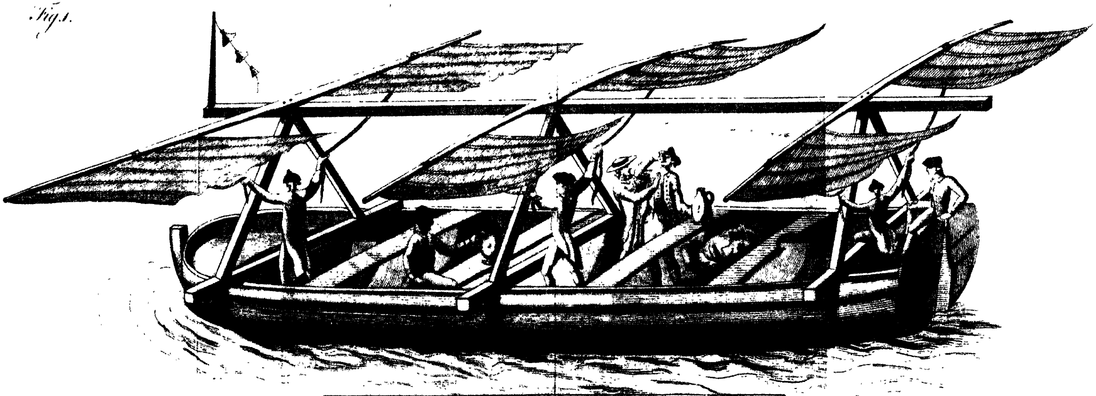
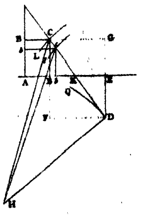


Fig. 2.



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Page.	Line.	
478.	18.	*for* brighter *read* leſs bright

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289.	8.	*for* the *read* thoſe
290.	11.	*dele* rather
291.	8.	*for* the *read* theſe
329.	7.	*for* 1784 *read* 1783
341.	23.	*for* , firſt, *read* fixed
342.	12.	*for* than *read* that
345.	1.	*after* from *inſert* their
365.	11.	*for* I *read* K

In plate XIV. fig. 2. the letter *a* ſhould be inſerted at the bottom, and the letter *b* at the top, of the dotted *interval* 19½, in the ſame manner as the letters *c* and *d* at the two extremes of the dotted *intervals* in fig. 3.

In plate XXI. fig. 2. at the lower angle of the diagram, in ſome copies, the letter K ſtands by miſtake inſtead of H.

I. A. R. I. 75.

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